SCIENCE

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CONTENTS

The Address of the President of the British Association for the Advancement of Science:—
Continuity: SIR OLIVER LODGE 379

A Summary of the Work of the U. S. Fisheries Marine Biological Station at Beaufort, N. C., during 1912: LEWIS RADCLIFFE 395

Scientific Notes and News 400

University and Educational News 401

Discussion and Correspondence:—

The Data of Inter-varietal and Inter-specific Competition in their Relation to the Problem of Natural Selection: DR. J. ARTHUR HARRIS. *Prepotency in Airedale Terriers:* WILLIAMS HAYNES. *Mitosis in the Adult Nerve Cells of the Colorado Beetle:* DR. W. M. SMALLWOOD, CHARLES G. ROGERS 402

Scientific Books:—

Sigma Xi Quarter Century Record and History: DR. MARCUS BENJAMIN. *Haas and Hill's Introduction to the Chemistry of Plant Products:* DR. ROSS AIKEN GORTNER 405

Special Articles:

The Organization of the Cell with respect to Permeability: PROFESSOR W. J. V. OSTERHOUT 408

The Society of American Bacteriologists. II.:

Sanitary Bacteriology; Soil Bacteriology: DR. A. PARKER HITCHENS 409

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CONTINUITY[1]

Natura non vincitur nisi parendo.

FIRST let me lament the catastrophe which has led to my occupying the chair here in this city. Sir William White was a personal friend of many here present, and I would that the citizens of Birmingham could have become acquainted with his attractive personality, and heard at first hand of the strenuous work which he accomplished in carrying out the behests of the empire in the construction of its first line of defence.

Although a British Association address is hardly an annual stocktaking, it would be improper to begin this year of office without referring to three more of our losses:—One that cultured gentleman, amateur of science in the best sense, who was chosen to preside over our jubilee meeting at York thirty-two years ago. Sir John Lubbock, first Baron Avebury, cultivated science in a spirit of pure enjoyment, treating it almost as one of the arts; and he devoted social and political energy to the welfare of the multitude of his fellows less fortunately situated than himself.

Through the untimely death of Sir George Darwin the world has lost a mathematical astronomer whose work on the tides and allied phenomena is a monument of power and achievement. So recently as our visit to South Africa he occupied the presidential chair.

By the third of our major losses, I mean the death of that brilliant mathematician of a neighboring nation who took so com-

[1] Address of the president of the British Association. Read at Birmingham, September 10, 1913.

prehensive and philosophic a grasp of the intricacies of physics, and whose eloquent though sceptical exposition of our laws and processes, and of the modifications entailed in them by recent advances, will be sure to attract still more widespread attention among all to whom the rather abstruse subject-matter is sufficiently familiar. I can not say that I find myself in agreement with all that Henri Poincaré wrote or spoke in the domain of physics, but no physicist can help being interested in his mode of presentation, and I may have occasion to refer, in passing, to some of the topics with which he dealt.

And now, eliminating from our purview, as is always necessary, a great mass of human activity, and limiting ourselves to a scrutiny on the side of pure science alone, let us ask what, in the main, is the characteristic of the promising though perturbing period in which we live. Different persons would give different answers, but the answer I venture to give is—rapid progress, combined with fundamental scepticism.

Rapid progress was not characteristic of the latter half of the nineteenth century—at least not in physics. Fine solid dynamical foundations were laid, and the edifice of knowledge was consolidated; but wholly fresh ground was not being opened up, and totally new buildings were not expected.

> In many cases the student was led to believe that the main facts of nature were all known, that the chances of any great discovery being made by experiment were vanishingly small, and that therefore the experimentalist's work consisted in deciding between rival theories, or in finding some small residual effect, which might add a more or less important detail to the theory.—Schuster.

With the realization of predicted ether waves in 1888, the discovery of X-rays in 1895, spontaneous radioactivity in 1896, and the isolation of the electron in 1898, expectation of further achievement became vivid; and novelties, experimental, theoretical and speculative, have been showered upon us ever since this century began. That is why I speak of rapid progress.

Of the progress I shall say little—there must always be some uncertainty as to which particular achievement permanently contributes to it; but I will speak about the fundamental scepticism.

Let me hasten to explain that I do not mean the well-worn and almost antique theme of theological scepticism: that controversy is practically in abeyance just now. At any rate the major conflict is suspended; the forts behind which the enemy has retreated do not invite attack; the territory now occupied by him is little more than his legitimate province. It is the scientific allies, now, who are waging a more or less invigorating conflict among themselves, with philosophers joining in. Meanwhile the ancient foe is biding his time and hoping that from the struggle something will emerge of benefit to himself. Some positions, he feels, were too hastily abandoned and may perhaps be retrieved; or, to put it without metaphor, it seems possible that a few of the things prematurely denied, because asserted on inconclusive evidence, may after all, in some form or other, have really happened. Thus the old theological bitterness is mitigated, and a temporizing policy is either advocated or instinctively adopted.

To illustrate the nature of the fundamental scientific or philosophic controversies to which I do refer, would require almost as many addresses as there are sections of the British Association, or at any rate as many as there are chief cities in Australia; and perhaps my successor in the chair will continue the theme; but, to exhibit my meaning very briefly, I may cite the kind of dominating controversies now extant, employing as far as possible

only a single word in each case so as to emphasize the necessary brevity and insufficiency of the reference.

In physiology the conflict ranges round *vitalism.* (My immediate predecessor dealt with the subject at Dundee.)

In chemistry the debate concerns *atomic structure.* (My penultimate predecessor is well aware of pugnacity in that region.)

In biology the dispute is on the laws of *inheritance.* (My successor is sure to deal with this subject; probably in a way not deficient in liveliness.)

And besides these major controversies, debate is active in other sections:

In education, *curricula* generally are being overhauled or fundamentally criticized, and revolutionary ideas are promulgated concerning the advantages of freedom for infants.

In economic and political science, or sociology, what is there that is not under discussion? Not property alone, nor land alone, but everything, —back to the garden of Eden and the interrelations of men and women.

Lastly, in the vast group of mathematical and physical sciences, "slurred over rather than summed up as Section A," present-day scepticism concerns what, if I had to express it in one word, I should call *continuity.* The full meaning of this term will hardly be intelligible without explanation, and I shall discuss it presently.

Still more fundamental and deep-rooted than any of these sectional debates, however, a critical examination of scientific foundations generally is going on; and a kind of philosophic scepticism is in the ascendant, resulting in a mistrust of purely intellectual processes and in a recognition of the limited scope of science.

For science is undoubtedly an affair of the intellect, it examines everything in the cold light of reason; and that is its strength. It is a commonplace to say that science must have no likes or dislikes, must aim only at truth; or as Bertrand Russell well puts it:

> The kernel of the scientific outlook is the refusal to regard our own desires, tastes and interests as affording a key to the understanding of the world.

This exclusive single-eyed attitude of science is its strength; but, if pressed beyond the positive region of usefulness into a field of dogmatic negation and philosophizing, it becomes also its weakness. For the nature of man is a large thing, and intellect is only a part of it: a recent part too, which therefore necessarily, though not consciously, suffers from some of the defects of newness and crudity, and should refrain from imagining itself the whole—perhaps it is not even the best part—of human nature.

The fact is that some of the best things are, by abstraction, excluded from science, though not from literature and poetry; hence perhaps an ancient mistrust or dislike of science, typified by the Promethean legend. Science is systematized and *metrical* knowledge, and in regions where measurement can not be applied it has small scope; or, as Mr. Balfour said the other day at the opening of a new wing of the National Physical Laboratory:

> Science depends on measurement, and things not measurable are therefore excluded, or tend to be excluded, from its attention. But life and beauty and happiness are not measurable.

And then characteristically he adds:

> If there could be a unit of happiness, politics might begin to be scientific.

Emotion and intuition and instinct are immensely older than science, and in a comprehensive survey of existence they can not be ignored. Scientific men may

rightly neglect them, in order to do their proper work, but philosophers can not.

So philosophers have begun to question some of the larger generalizations of science, and to ask whether in the effort to be universal and comprehensive we have not extended our laboratory inductions too far. The conservation of energy, for instance—is it always and everywhere valid; or may it under some conditions be disobeyed? It would seem as if the second law of thermodynamics must be somewhere disobeyed—at least if the age of the universe is both ways infinite—else the final consummation would have already arrived.

Not by philosophers only, but by scientific men also, ancient postulates are being pulled up by the roots. Physicists and mathematicians are beginning to consider whether the long known and well-established laws of mechanics hold true everywhere and always, or whether the Newtonian scheme must be replaced by something more modern, something to which Newton's laws of motion are but an approximation.

Indeed a whole system of non-Newtonian mechanics has been devised, having as its foundation the recently discovered changes which must occur in bodies moving at speeds nearly comparable with that of light. It turns out in fact that both shape and mass are functions of velocity. As the speed increases the mass increases and the shape is distorted, though under ordinary conditions only to an infinitesimal extent.

So far I agree; I agree with the statement of fact; but I do not consider it so revolutionary as to overturn Newtonian mechanics. After all, a variation of mass is familiar enough, and it would be a great mistake to say that Newton's second law breaks down merely because mass is not constant. A raindrop is an example of variable mass; or the earth may be, by reason of meteoric dust; or the sun, by reason of radio-activity; or a locomotive, by reason of the emission of steam. In fact, variable masses are the commonest, for friction may abrade any moving body to a microscopic extent.

That mass is constant is only an approximation. That mass is equal to ratio of force and acceleration is a definition, and can be absolutely accurate. It holds perfectly even for an electron with a speed near that of light; and it is by means of Newton's second law that the variation of mass with velocity has been experimentally observed and compared with theory.

I urge that we remain with, or go back to, Newton. I see no reason against retaining all Newton's laws, discarding nothing, but supplementing them in the light of further knowledge.

Even the laws of geometry have been overhauled, and Euclidean geometry is seen to be but a special case of more fundamental generalizations. How far they apply to existing space, and how far time is a reality or an illusion, and whether it can in any sense depend on the motion or the position of an observer: all these things in some form or other are discussed.

The conservation of matter also, that main-mast of nineteenth century chemistry, and the existence of the ether of space, that sheet-anchor of nineteenth century physics—do they not sometimes seem to be going by the board?

Professor Schuster, in his American lectures, commented on the modern receptive attitude as follows:-

> The state of plasticity and flux—a healthy state, in my opinion—in which scientific thought of the present day adapts itself to almost any novelty, is illustrated by the complacency with which the most cherished tenets of our fathers are being abandoned. Though it was never an article of orthodox faith that chemical elements were immutable and would not some day be resolved into

simpler constituents, yet the conservation of mass seemed to lie at the very foundation of creation. But nowadays the student finds little to disturb him, perhaps too little, in the idea that mass changes with velocity; and he does not always realize the full meaning of the consequences which are involved.

This readiness to accept and incorporate new facts into the scheme of physics may have led to perhaps an undue amount of scientific scepticism, in order to right the balance.

But a still deeper variety of comprehensive scepticism exists, and it is argued that all our laws of nature, so laboriously ascertained and carefully formulated, are but conventions after all, not truths: that we have no faculty for ascertaining real truth, that our intelligence was not evolved for any such academic purpose; that all we can do is to express things in a form convenient for present purposes and employ that mode of expression as a tentative and pragmatically useful explanation.

Even *explanation,* however, has been discarded as too ambitious by some men of science, who claim only the power to describe. They not only emphasize the *how* rather than the *why*—as is in some sort inevitable, since explanations are never ultimate—but are satisfied with very abstract propositions, and regard mathematical equations as preferable to, because safer than, mechanical analogies or models.

To use an acute and familiar expression of Gustav Kirchhoff, it is the object of science to *describe* natural phenomena, not to *explain* them. When we have expressed by an equation the correct relationship between different natural phenomena we have gone as far as we safely can, and if we go beyond we are entering on purely speculative ground.

But the modes of statement preferred by those who distrust our power of going correctly into detail are far from satisfactory. Professor Schuster describes and comments on them thus:

Vagueness, which used to be recognized as our great enemy, is now being enshrined as an idol to be worshipped. We may never know what constitutes atoms, or what is the real structure of the ether; why trouble, therefore, it is said, to find out more about them. Is it not safer, on the contrary, to confine ourselves to a general talk on entropy, luminiferous vectors and undefined symbols expressing vaguely certain physical relationships? What really lies at the bottom of the great fascination which these new doctrines exert on the present generation is sheer cowardice; the fear of having its errors brought home to it. . . .

I believe this doctrine to be fatal to a healthy development of science. Granting the impossibility of penetrating beyond the most superficial layers of observed phenomena, I would put the distinction between the two attitudes of mind in this way: One glorifies our ignorance, while the other accepts it as a regrettable necessity.

In further illustration of the modern sceptical attitude, I quote from Poincaré:

Principles are conventions and definitions in disguise. They are, however, deduced from experimental laws, and these laws have, so to speak, been erected into principles to which our mind attributes an absolute value. . . .

The fundamental propositions of geometry, for instance Euclid's postulate, are only conventions; and it is quite as unreasonable to ask if they are true or false as to ask if the metric system is true or false. Only, these conventions are convenient. . . .

Whether the ether exists or not matters little—let us leave that to the metaphysicians; what is essential for us is that everything happens as if it existed, and that this hypothesis is found to be suitable for the explanation of phenomena. After all, have we any other reason for believing in the existence of material objects? That, too, is only a convenient hypothesis.

As an antidote against over-pressing these utterances I quote from Sir J. Larmor's preface:

There has been of late a growing trend of opinion, prompted in part by general philosophical views, in the direction that the theoretical constructions of physical science are largely factitious, that instead of presenting a valid image of the relations of things on which further progress can be based, they are still little better than a mirage. . . .

> The best method of abating this scepticism is to become acquainted with the real scope and modes of application of conceptions which, in the popular language of superficial exposition—and even in the unguarded and playful paradox of their authors, intended only for the instructed eye —often look bizarre enough.

One thing is very notable, that it is closer and more exact knowledge that has led to the kind of scientific scepticism now referred to; and that the simple laws on which we used to be working were thus simple and discoverable because the full complexity of existence was tempered to our ken by the roughness of our means of observation.

Kepler's laws are not accurately true, and if he had had before him all the data now available he could hardly have discovered them. A planet does not really move in an ellipse but in a kind of hypocycloid, and not accurately in that either.

So it is also with Boyle's law, and the other simple laws in physical chemistry. Even Van der Waals's generalization of Boyle's law is only a further approximation.

In most parts of physics simplicity has sooner or later to give place to complexity: though certainly I urge that the simple laws were true, and are still true, as far as they go, their inaccuracy being only detected by further real discovery. The reason they are departed from becomes known to us; the law is not really disobeyed, but is modified through the action of a known additional cause. Hence it is all in the direction of progress.

It is only fair to quote Poincaré again, now that I am able in the main to agree with him:

> Take for instance the laws of reflection. Fresnel established them by a simple and attractive theory which experiment seemed to confirm. Subsequently, more accurate researches have shown that this verification was but approximate; traces of elliptic polarization were detected everywhere. But it is owing to the first approximation that the cause of these anomalies was found, in the existence of a transition layer; and all the essentials of Fresnel's theory have remained. We can not help reflecting that all these relations would never have been noted if there had been doubt in the first place as to the complexity of the objects they connect. Long ago it was said: If Tycho had had instruments ten times as precise, we would never have had a Kepler, or a Newton, or astronomy. It is a misfortune for a science to be born too late, when the means of observation have become too perfect. That is what is happening at this moment with respect to physical chemistry; the founders are hampered in their general grasp by third and fourth decimal places; happily they are men of robust faith. As we get to know the properties of matter better we see that continuity reigns. . . . It would be difficult to justify [the belief in continuity] by apodeictic reasoning, but without [it] all science would be impossible.

Here he touches on my own theme, *continuity;* for, if we had to summarize the main trend of physical controversy at present, I feel inclined to urge that it largely turns on the question as to which way ultimate victory lies in the fight between continuity and discontinuity.

On the surface of nature at first we see discontinuity; objects detached and countable. Then we realize the air and other media, and so emphasize continuity and flowing quantities. Then we detect atoms and numerical properties, and discontinuity once more makes its appearance. Then we invent the ether and are impressed with continuity again. But this is not likely to be the end; and what the ultimate end will be, or whether there is an ultimate end, is a question difficult to answer.

The modern tendency is to emphasize the discontinuous or atomic character of everything. Matter has long been atomic, in the same sense as anthropology is atomic; the unit of matter is the atom, as the unit of humanity is the individual. Whether men or women or children—they can be counted

as so many "souls." And atoms of matter can be counted too.

Certainly however there is an illusion of continuity. We recognize it in the case of water. It appears to be a continuous medium, and yet it is certainly molecular. It is made continuous again, in a sense, by the ether postulated in its pores; for the ether is essentially continuous. Though Osborne Reynolds, it is true, invented a discontinuous or granular ether, on the analogy of the seashore. The sands of the sea, the hairs of the head, the descendants of a patriarch, are typical instances of numerable, or rather of innumerable, things. The difficulty of enumerating them is not that there is nothing to count, but merely that the things to be counted are very numerous. So are the atoms in a drop of water—they outnumber the drops in an Atlantic Ocean—and, during the briefest time of stating their number, fifty millions or so may have evaporated; but they are as easy to count as the grains of sand on a shore.

The process of counting is evidently a process applicable to discontinuities, *i. e.*, to things with natural units; you can count apples and coins, and days and years, and people and atoms. To apply number to a continuum you must first cut it up into artificial units; and you are always left with incommensurable fractions. Thus only is it that you can deal numerically with such continuous phenomena as the warmth of a room, the speed of a bird, the pull of a rope or the strength of a current.

But how, it may be asked, does discontinuity apply to number? The natural numbers, 1, 2, 3, etc., are discontinuous enough, but there are fractions to fill up the interstices; how do we know that they are not really connected by these fractions, and so made continuous again?

(By number I always mean commensurable number; incommensurables are not numbers: they are just what can not be expressed in numbers. The square root of 2 is not a number, though it can be readily indicated by a length. Incommensurables are usual in physics and are frequent in geometry; the conceptions of geometry are essentially continuous. It is clear, as Poincaré says, that "if the points whose coordinates are commensurable were alone regarded as real, the in-circle of a square and the diagonal of the square would not intersect, since the coordinates of the points of intersection are incommensurable.")

I want to explain how commensurable fractions do not connect up numbers, nor remove their discontinuity in the least. The divisions on a foot rule, divided as closely as you please, represent commensurable fractions, but they represent none of the length. No matter how numerous they are, all the length lies between them; the divisions are mere partitions and have consumed none of it; nor do they connect up with each other, they are essentially discontinuous. The interspaces are infinitely more extensive than the barriers which partition them off from one another; they are like a row of compartments with infinitely thin walls. All the incommensurables lie in the interspaces; the compartments are full of them, and they are thus infinitely more numerous than the numerically expressible magnitudes. Take any point of the scale at random, that point will certainly lie in an interspace: it will not lie on a division, for the chances are infinity to 1 against it.

Accordingly incommensurable quantities are the rule in physics. Decimals do not in practise terminate or circulate, in other words vulgar fractions do not accidentally occur in any measurements, for this would mean infinite accuracy. We proceed to as

many places of decimals as correspond to the order of accuracy aimed at.

Whenever, then, a commensurable number is really associated with any natural phenomenon, there is necessarily a noteworthy circumstance involved in the fact, and it means something quite definite and ultimately ascertainable. Every discontinuity that can be detected and counted is an addition to knowledge. It not only means the discovery of natural units instead of being dependent on artificial ones, but it throws light also on the nature of phenomena themselves.

For instance:

The ratio between the velocity of light and the inverted square root of the product of the electric and magnetic constants was discovered by Clerk Maxwell to be 1; and a new volume of physics was by that discovery opened.

Dalton found that chemical combination occurred between quantities of different fractional numbers; and the atomic theory of matter sprang into substantial though at first infantile existence.

The hypothesis of Prout, which in some modified form seems likely to be substantiated, is that all atomic weights are commensurable numbers; in which case there must be a natural fundamental unit underlying, and in definite groups composing, the atoms of every form of matter.

The small number of degrees of freedom of a molecule, and the subdivision of its total energy into equal parts corresponding thereto, is a theme not indeed without difficulty but full of importance. It is responsible for the suggestion that energy too may be atomic!

Mendelejeff's series again, or the detection of a natural grouping of atomic weights in families of seven, is another example of the significance of number.

Electricity was found by Faraday to be numerically connected with quantity of matter; and the atom of electricity began its hesitating but now brilliant career.

Electricity itself—*i. e.*, electric charge—strangely enough has proved itself to be atomic. There is a natural unit of electric charge, as suspected by Faraday and Maxwell and named by Johnstone Stoney. Some of the electron's visible effects were studied by Crookes in a vacuum; and its weighing and measuring by J. J. Thomson were announced to the British Association meeting at Dover in 1899, a fitting prelude to the twentieth century.

An electron is the natural unit of negative electricity, and it may not be long before the natural unit of positive electricity is found too. But concerning the nature of the positive unit there is at present some division into opposite camps. One school prefers to regard the unit of positive electricity as a homogeneous sphere, the size of an atom, in which electrons revolve in simple harmonic orbits and constitute nearly the whole effective mass. Another school, while appreciative of the simplicity and ingenuity and beauty of the details of this conception, and the skill with which it has been worked out, yet thinks the evidence more in favor of a minute central positive nucleus, or nucleus-group, of practically atomic mass; with electrons, larger—*i. e.*, less concentrated—and therefore less massive than itself, revolving round it in astronomical orbits. While from yet another point of view it is insisted that positive and negative electrons can only differ skew-symmetrically, one being like the image of the other in a mirror, and that the mode in which they are grouped to form an atom remains for future discovery. But no one doubts that electricity is ultimately atomic.

Even magnetism has been suspected of being atomic, and its hypothetical unit has

been named in advance the magneton: but I confess that here I have not been shaken out of the conservative view.

We may express all this as an invasion of number into unsuspected regions.

Biology may be said to be becoming atomic. It has long had natural units in the shape of cells and nuclei, and some discontinuity represented by body-boundaries and cell-walls; but now, in its laws of heredity as studied by Mendel, number and discontinuity are strikingly apparent among the reproductive cells, and the varieties of offspring admit of numerical specification and prediction to a surprising extent: while modification by continuous variation, which seemed to be of the essence of Darwinism, gives place to, or at least is accompanied by, mutation, with finite and considerable and in appearance discontinuous change.

So far from nature not making jumps, it becomes doubtful if she does anything else. Her hitherto placid course, more closely examined, seems to look like a kind of steeplechase.

Yet undoubtedly continuity is the backbone of evolution, as taught by all biologists—no artificial boundaries or demarcations between species—a continuous chain of heredity from far below the amœba up to man. Actual continuity of undying germplasm, running through all generations, is taught likewise; though a strange discontinuity between this persistent element and its successive accessory body-plasms—a discontinuity which would convert individual organisms into mere temporary accretions or excretions, with no power of influencing or conveying experience to their generating cells—is advocated by one school.

Discontinuity does not fail to exercise fascination even in pure mathematics. Curves are invented which have no tangent or differential coefficient, curves which consist of a succession of dots or of twists; and the theory of commensurable numbers seems to be exerting a dominance over philosophic mathematical thought as well as over physical problems.

And not only these fairly accepted results are prominent, but some more difficult and unexpected theses in the same direction are being propounded, and the atomic character of energy is advocated. We had hoped to be honored by the presence of Professor Planck, whose theory of the *quantum,* or indivisible unit or atom of energy, excites the greatest interest, and by some is thought to hold the field.

Then again radiation is showing signs of becoming atomic or discontinuous. The corpuscular theory of radiation is by no means so dead as in my youth we thought it was. Some radiation is certainly corpuscular, and even the etherial kind shows indications, which may be misleading, that it is spotty, or locally concentrated into points, as if the wave-front consisted of detached specks or patches; or, as J. J. Thomson says, "the wave-front must be more analogous to bright specks on a dark ground than to a uniformly illuminated surface," thus suggesting that the ether may be fibrous in structure, and that a wave runs along lines of electric force, as the genius of Faraday surmised might be possible, in his "Thoughts on Ray Vibrations." Indeed Newton guessed something of the same kind, I fancy, when he superposed ether-pulses on his corpuscles.

Whatever be the truth in this matter, a discussion on radiation, of extreme weight and interest, though likewise of great profundity and technicality, is expected on Friday in Section A. We welcome Professor Lorentz, Dr. Arrhenius, Professor Langevin, Professor Pringsheim and others, some of whom have been specially invited to England because of the impor-

tant contributions which they have made to the subject-matter of this discussion.

Why is so much importance attached to radiation? Because it is the best-known and longest-studied link between matter and ether, and the only property we are acquainted with that affects the unmodified great mass of ether alone. Electricity and magnetism are associated with the modifications or singularities called electrons: most phenomena are connected still more directly with matter. Radiation, however, though excited by an accelerated electron, is subsequently let loose in the ether of space, and travels as a definite thing at a measurable and constant pace—a pace independent of everything so long as the ether is free, unmodified and unloaded by matter. Hence radiation has much to teach us, and we have much to learn concerning its nature.

How far can the analogy of granular, corpuscular, countable, atomic or discontinuous things be pressed? There are those who think it can be pressed very far. But to avoid misunderstanding let me state, for what it may be worth, that I myself am an upholder of *ultimate* continuity, and a fervent believer in the ether of space.

We have already learned something about the ether; and although there may be almost as many varieties of opinion as there are people qualified to form one, in my view we have learned as follows:

The ether is the universal connecting medium which binds the universe together, and makes it a coherent whole instead of a chaotic collection of independent isolated fragments. It is the vehicle of transmission of all manner of force, from gravitation down to cohesion and chemical affinity; it is therefore the storehouse of potential energy.

Matter moves, but ether is strained.

What we call elasticity of matter is only the result of an alteration of configuration due to movement and readjustment of particles, but all the strain and stress are in the ether. The ether itself does not move, that is to say it does not move in the sense of locomotion, though it is probably in a violent state of rotational or turbulent motion in its smallest parts; and to that motion its exceeding rigidity is due.

As to its density, it must be far greater than that of any form of matter, millions of times denser than lead or platinum. Yet matter moves through it with perfect freedom, without any friction or viscosity. There is nothing paradoxical in this: viscosity is not a function of density; the two are not necessarily connected. When a solid moves through an alien fluid it is true that it acquires a spurious or apparent extra inertia from the fluid it displaces; but in the case of matter and ether, not only is even the densest matter excessively porous and discontinuous, with vast interspaces in and among the atoms, but the constitution of matter is such that there appears to be no displacement in the ordinary sense at all; the ether is itself so modified as to *constitute* the matter in some way. Of course that portion moves, its inertia is what we observe, and its amount depends on the potential energy in its associated electric field, but the motion is not like that of a foreign body, it is that of some inherent and merely individualized portion of the stuff itself. Certain it is that the ether exhibits no trace of viscosity.[2]

Matter in motion, ether under strain, constitute the fundamental concrete things we have to do with in physics. The first

[2] For details of my experiment on this subject see *Phil Trans. Roy. Soc.* for 1893 and 1897; or a very abbreviated reference to it, and to the other matters above mentioned, in my small book, "The Ether of Space."

pair represent kinetic energy, the second potential energy; and all the activities of the material universe are represented by alternations from one of these forms to the other.

Whenever this transference and transformation of energy occur, work is done, and some effect is produced, but the energy is never diminished in quantity: it is merely passed on from one body to another, always from ether to matter or *vice versa* —except in the case of radiation, which simulates matter—and from one form to another.

The forms of energy can be classified as either a translation, a rotation or a vibration of pieces of matter of different sizes, from stars and planets down to atoms and electrons; or else an etherial strain which in various different ways is manifested by the behavior of such masses of matter as appeal to our senses.[3]

Some of the facts responsible for the suggestion that energy is atomic seem to me to depend on the discontinuous nature of the structure of a material atom, and on the high velocity of its constituent particles. The apparently discontinuous emission of radiation is, I believe, due to features in the real discontinuity of matter. Disturbances inside an atom appear to be essentially catastrophic; a portion is liable to be ejected with violence. There appears to be a critical velocity below which ejection does not take place; and, when it does, there also occurs a sudden rearrangement of parts which is presumably responsible for some perceptible etherial radiation. Hence it is, I suppose, that radiation comes off in gushes or bursts; and hence it appears to consist of indivisible units. The occasional phenomenon of new stars, as compared with the steady orbital motion of the millions of recognized bodies, may be suggested as an astronomical analogue.

The hypothesis of *quanta* was devised to reconcile the law that the energy of a group of colliding molecules must in the long run be equally shared among all their degrees of freedom, with the observed fact that the energy is really shared into only a small number of equal parts. For if vibration-possibilities have to be taken into account, the number of degrees of molecular freedom must be very large, and energy shared among them ought soon to be all frittered away; whereas it is not. Hence the idea is suggested that minor degrees of freedom are initially excluded from sharing the energy, because they can not be supplied with less than one atom of it.

I should prefer to express the fact by saying that the ordinary encounters of molecules are not of a kind able to excite atomic vibrations, or in any way to disturb the ether. Spectroscopic or luminous vibrations of an atom are excited only by an exceptionally violent kind of collision, which may be spoken of as chemical clash; the ordinary molecular orbital encounters, always going on at the rate of millions a second, are ineffective in that respect, except in the case of phosphorescent or luminescent substances. That common molecular deflexions *are* ineffective is certain, else all the energy would be dissipated or transferred from matter into the ether; and the reasonableness of their radiative inefficiency is not far to seek, when we consider the comparatively leisurely character of molecular movements, at speeds comparable with the velocity of sound. Admittedly, however, the effective rigidity of molecules must be complete, otherwise the sharing of energy must ultimately occur. They do not seem able

[3] See, in the *Philosophical Magazine* for 1879, my article on "A Classification of the Forms of Energy."

to be set vibrating by anything less than a certain minimum stimulus; and that is the basis for the theory of *quanta*.

Quantitative applications of Planck's theory, to elucidate the otherwise shaky stability of the astronomically constituted atom, have been made; and the agreement between results so calculated and those observed, including a determination of series of spectrum lines, is very remarkable. One of the latest contributions to this subject is a paper by Dr. Bohr in the *Philosophical Magazine* for July this year.

To show that I am not exaggerating the modern tendency towards discontinuity, I quote, from M. Poincaré's "Dernières Pensées," a proposition which he announces in italics as representing a form of Professor Planck's view of which he apparently approves:

> A physical system is susceptible of a finite number only of distinct conditions; it jumps from one of these conditions to another without passing through a continuous series of intermediate conditions.

Also this from Sir Joseph Larmor's preface to Poincaré's "Science and Hypothesis":

> Still more recently it has been found that the good Bishop Berkeley's logical jibes against the Newtonian ideas of fluxions and limiting ratios can not be adequately appeased in the rigorous mathematical conscience, until our apparent continuities are resolved mentally into discrete aggregates which we only partially apprehend. The irresistible impulse to atomize everything thus proves to be not merely a disease of the physicist: a deeper origin, in the nature of knowledge itself, is suggested.

One very valid excuse for this prevalent attitude is the astonishing progress that has been made in actually seeing or almost seeing the molecules, and studying their arrangement and distribution.

The laws of gases have been found to apply to emulsions and to fine powders in suspension, of which the Brownian movement has long been known. This movement is caused by the orthodox molecular bombardment, and its average amplitude exactly represents the theoretical mean free path calculated from the "molecular weight" of the relatively gigantic particles. The behavior of these microscopically visible masses corresponds closely and quantitatively with what could be predicted for them as fearfully heavy atoms, on the kinetic theory of gases; they may indeed be said to constitute a gas with a gram-molecule as high as 200,000 tons; and, what is rather important as well as interesting, they tend visibly to verify the law of equipartition of energy even in so extreme a case, when that law is properly stated and applied.

Still more remarkable, the application of X-rays to display the arrangement of molecules in crystals, and ultimately the arrangement of atoms in molecules, as initiated by Professor Laue with Drs. Friedrich and Knipping, and continued by Professor Bragg and his son and by Dr. Tutton, constitute a series of researches of high interest and promise. By this means many of the theoretical anticipations of our countryman, Mr. William Barlow, and—working with him—Professor Pope, as well as of those distinguished crystallographers von Groth and von Fedorow, have been confirmed in a striking way. These brilliant researches, which seem likely to constitute a branch of physics in themselves, and which are being continued by Messrs. Moseley and C. G. Darwin, and by Mr. Keene and others, may be called an apotheosis of the atomic theory of matter.

One other controversial topic I shall touch upon in the domain of physics, though I shall touch upon it lightly, for it is not a matter for easy reference as yet. If the *principle of relativity* in an extreme

sense establishes itself, it seems as if even time would become discontinuous and be supplied in atoms, as money is doled out in pence or centimes instead of continuously —in which case our customary existence will turn out to be no more really continuous than the events on a kinematograph screen—while that great agent of continuity, the ether of space, will be relegated to the museum of historical curiosities.

In that case differential equations will cease to represent the facts of nature, they will have to be replaced by finite differences, and the most fundamental revolution since Newton will be inaugurated.

Now in all the debatable matters of which I have indicated possibilities I want to urge a conservative attitude. I accept the new experimental results on which some of these theories—such as the principle of relativity—are based, and am profoundly interested in them, but I do not feel that they are so revolutionary as their propounders think. I see a way to retain the old and yet embrace the new, and I urge moderation in the uprooting and removal of landmarks.

And of these the chief is continuity. I can not imagine the exertion of mechanical force across empty space, no matter how minute; a continuous medium seems to me essential. I can not admit discontinuity in either space or time, nor can I imagine any sort of experiment which would justify such a hypothesis. For surely we must realize that we know nothing experimental of either space or time, we can not modify them in any way. We make experiments on bodies, and only on bodies, using "body" as an exceedingly general term.

We have no reason to postulate anything but continuity for space and time. We cut them up into conventional units for convenience' sake, and those units we can count; but there is really nothing atomic or countable about the things themselves. We can count the rotations of the earth, or the revolutions of an electron, or the vibrations of a pendulum, or the waves of light. All these are concrete and tractable physical entities; but space and time are ultimate data, abstractions based on experience. We know them through motion, and through motion only, and motion is essentially continuous. We ought clearly to discriminate between things themselves and our mode of measuring them. Our measures and perceptions may be affected by all manner of incidental and trivial causes, and we may get confused or hampered by our own movement; but there need be no such complication in things themselves, any more than a landscape is distorted by looking at it through an irregular window-pane or from a traveling coach. It is an ancient and discarded fable that complications introduced by the motion of an observer are real complications belonging to the outer universe.

Very well, then, what about the ether, is that in the same predicament? Is that an abstraction, or a mere convention, or is it a concrete physical entity on which we can experiment?

Now it has to be freely admitted that it is exceedingly difficult to make experiments on the ether. It does not appeal to sense, and we know no means of getting hold of it. The one thing we know metrical about it is the velocity with which it can transmit transverse waves. That is clear and definite, and thereby to my judgment it proves itself a physical agent; not indeed tangible or sensible, but yet concretely real.

But it does elude our laboratory grasp. If we rapidly move matter through it, hoping to grip it and move it too, we fail: there is no mechanical connection. And even if we experiment on light we fail too. So long as transparent matter is moving

relatively to us, light can be affected inside that matter; but when matter is relatively stationary to matter nothing observable takes place, however fast things may be moving, so long as they move together.

Hence arises the idea that motion with respect to ether is meaningless: and the fact that only relative motion of pieces of matter with respect to each other has so far been observed is the foundation of the principle of relativity. It sounds simple enough as thus stated, but in its developments it is an ingenious and complicated doctrine embodying surprising consequences which have been worked out by Professor Einstein and his disciples with consummate ingenuity.

What have I to urge against it? Well, in the first place, it is only in accordance with common sense that no effect of the first order can be observed without relative motion of matter. An ether-stream through our laboratories is optically and electrically undetectable, at least as regards first-order observation; this is clearly explained for general readers in my book, "The Ether of Space," chapter IV. But the principle of relativity says more than that, it says that no effect of any order of magnitude can ever be observed without the relative motion of matter.

The truth underlying this doctrine is that absolute motion without reference to *anything* is unmeaning. But the narrowing down of "anything" to mean any piece of matter is illegitimate. The nearest approach to absolute motion that we can physically imagine is motion through or with respect to the ether of space. It is natural to assume that the ether is on the whole stationary and to use it as a standard of rest; in that sense motion with reference to it may be called absolute, but in no other sense.

The principle of relativity claims that we can never ascertain such motion: in other words, it practically or pragmatically denies the existence of the ether. Every one of our scientifically observed motions, it says, are of the same nature as our popularly observed ones, viz., motion of pieces of matter relatively to each other; and that is all that we can ever know. Everything goes on—says the principle of relativity—as if the ether did not exist.

Now the facts are that no motion with reference to the ether alone has ever yet been observed: there are always curious compensating effects which just cancel out the movement-terms and destroy or effectively mask any phenomenon that might otherwise be expected. When matter moves past matter observation can be made; but, even so, no consequent locomotion of ether, outside the actually moving particles, can be detected.

(It is sometimes urged that rotation is a kind of absolute motion that can be detected, even in isolation. It can so be detected, as Newton pointed out; but in cases of rotation matter on one side the axis is moving in the opposite direction to matter on the other side of the axis; hence rotation involves relative material motion, and therefore can be observed.)

To detect motion through ether we must use an etherial process. We may use radiation, and try to compare the speeds of light along or across the motion; or we might try to measure the speed, first with the motion and then against it. But how are we to make the comparison? If the time of emission from a distant source is given by a distant clock, that clock must be observed through a telescope, that is, by a beam of light; which is plainly a compensating process. Or the light from a neighboring source can be sent back to us by a distant mirror; when again there will be compensation. Or the starting of light

from a distant terrestrial source may be telegraphed to us, either with a wire or without; but it is the ether that conveys the message in either case, so again there will be compensation. Electricity, magnetism and light are all effects of the ether.

Use cohesion, then; have a rod stretching from one place to another, and measure that. But cohesion is transmitted by the ether too, if, as believed, it is the universal binding medium. Compensation is likely; compensation can, on the electrical theory of matter, be predicted.

Use some action not dependent on ether, then. Very well, where shall we find it?

To illustrate the difficulty I will quote a sentence from Sir Joseph Larmor's paper before the International Congress of Mathematicians at Cambridge last year:

> If it is correct to say with Maxwell that all radiation is an electrodynamic phenomenon, it is equally correct to say with him that all electrodynamic relations between material bodies are established by the operation, on the molecules of those bodies, of fields of force which are propagated in free space as radiation and in accordance with the laws of radiation, from one body to the other.

The fact is we are living in an epoch of some very comprehensive generalizations. The physical discovery of the twentieth century, so far, is the electrical theory of matter. This is the great new theory of our time; it was referred to, in its philosophical aspect, by Mr. Balfour in his presidential address at Cambridge in 1904. We are too near it to be able to contemplate it properly; it has still to establish itself and to develop in detail, but I anticipate that in some form or other it will prove true.[4]

Here is a briefest possible summary of the first chapter (so to speak) of the electrical theory of matter.

1. Atoms of matter are composed of electrons—of positive and negative electric charges.

2. Atoms are bound together into molecules by chemical affinity, which is intense electrical attraction at ultra-minute distances.

3. Molecules are held together by cohesion, which I for one regard as residual or differential chemical affinity over molecular distances.

4. Magnetism is due to the locomotion of electrons. There is no magnetism without an electric current, atomic or otherwise. There is no electric current without a moving electron.

5. Radiation is generated by every accelerated electron, in amount proportional to the square of its acceleration; and there is no other kind of radiation, except indeed a corpuscular kind; but this depends on the velocity of electrons and therefore again can only be generated by their acceleration.

The theory is bound to have curious consequences; and already it has contributed to some of the uprooting and uncertainty that I speak of. For, if it be true, every material interaction will be electrical, *i. e.*, etherial; and hence arises our difficulty. Every kind of force is transmitted by the ether, and hence, so long as all our apparatus is traveling together at one and the same pace, we have no chance of detecting the motion. That is the strength of the principle of relativity. The changes are not zero, but they cancel each other out of observation.

Many forms of statement of the famous Michelson-Morley experiment are misleading. It is said to prove that the time taken by light to go with the ether stream is the same as that taken to go against or across it. It does not show that. What it shows

[4] For a general introductory account of the electrical theory of matter my Romanes lecture for 1903 (Clarendon Press), may be referred to.

is that the time taken by light to travel to and fro on a measured interval fixed on a rigid block of matter is independent of the aspect of that block with respect to any motion of the earth through space. A definite and most interesting result: but it may be, and often is, interpreted loosely and too widely.

It is interpreted too widely, as I think, when Professor Einstein goes on to assume that no non-relative motion of matter can be ever observed even when *light* is brought into consideration. The relation of light to matter is very curious. The wave front of a progressive wave simulates many of the properties of matter. It has energy, it has momentum, it exerts force, it sustains reaction. It has been described as a portion of the mass of a radiating body—which gives it a curiously and unexpectedly corpuscular "feel." But it has a definite velocity. Its velocity in space relative to the ether is an absolute constant independent of the motion of the source. This would not be true for corpuscular light.

Hence I hold that here is something with which our own motion may theoretically be compared; and I predict that our motion through the ether will some day be detected by help of this very fact—by comparing our speed with that of light: though the old astronomical aberration, which seemed to make the comparison easy, failed to do so quite simply, because it is complicated by the necessity of observing the position of a distant source, in relation to which the earth is moving. If the source and observer are moving together there is no possibility of observing aberration. Nevertheless I maintain that when matter is moving near a beam of light we may be able to detect the motion. For the velocity of light in space is no function of the velocity of the source, nor of matter near it; it is quite unaffected by source or receiver. Once launched it travels in its own way. If we are traveling to meet it, it will be arriving at us more quickly; if we travel away from it, it will reach us with some lag. And observation of the acceleration or retardation is made by aid of Jupiter's satellites. We have there the dial of a clock, to or from which we advance or recede periodically. It gains while we approach it, it loses while we recede from it, it keeps right time when we are stationary or only moving across the line of sight.

But then of course it does not matter whether Jupiter is standing still and we are moving, or *vice versa:* it is a case of relative motion of matter again. So it is if we observe a Doppler effect from the right- and left-hand limbs of the rotating sun. True, and if we are to permit no relative motion of matter we must use a terrestrial source, clamped to the earth as our receiver is. And now we shall observe nothing.

But not because there is nothing to observe. Lag must really occur if we are running away from the light, even though the source is running after us at the same pace, unless we make the assumption—true only for corpuscular light—that the velocity of light is not an absolute thing, but is dependent on the speed of the source. With corpuscular light there is nothing to observe; with wave light there is something, but we can not observe it.

But if the whole solar system is moving through the ether I see no reason why the relative ether drift should not be observed by a differential residual effect in connection with Jupiter's satellites or the right and left limbs of the sun. The effect must be too small to observe without extreme precision, but theoretically it ought to be there. Inasmuch, however, as relative motion of matter with respect to the observer

is involved in these effects, it may be held that the detection of a uniform drift of the solar system in this way is not contrary to the principle of relativity. It is contrary to some statements of that principle; and the cogency of those statements breaks down, I think, whenever they include the velocity of light; because there we really have something absolute (in the only sense in which the term can have a physical meaning) with which we can compare our own motions, when we have learned how.

But in ordinary astronomical translation —translation as of the earth in its orbit— all our instruments, all our standards, the whole contents of our laboratory, are moving at the same rate in the same direction; under those conditions we can not expect to observe anything. Clerk Maxwell went so far as to say that if every particle of matter simultaneously received a graduated blow so as to produce a given constant acceleration all in the same direction, we should be unaware of the fact. He did not then know all that we know about radiation. But apart from that, and limiting ourselves to comparatively slow changes of velocity, our standards will inevitably share whatever change occurs. So far as observation goes, everything will be practically as if no change had occurred at all —though that may not be the truth. All that experiment establishes is that there have so far always been compensations; so that the attempt to observe motion through the ether is being given up as hopeless.

Surely, however, the minute and curious compensations can not be accidental, they must be necessary? Yes, they are necessary; and I want to say why. Suppose the case were one of measuring thermal expansion; and suppose everything had the same temperature and the same expansibility; our standards would contract or expand with everything else, and we could observe nothing; but expansion would occur nevertheless. That is obvious, but the following assertion is not so obvious. If everything in the universe had the same temperature, no matter what that temperature was, nothing would be visible at all; the external world so far as vision went, would not appear to exist. Visibility depends on radiation, on differential radiation. We must have differences to appeal to our senses, they are not constructed for uniformity.

It is the extreme omnipresence and uniformity and universal agency of the ether of space that makes it so difficult to observe. To observe anything you must have differences. If all actions at a distance are conducted at the same rate through the ether, the travel of none of them can be observed. Find something not conveyed by the ether and there is a chance. But then every physical action is transmitted by the ether, and in every case by means of its transverse or radiation-like activity.

Except perhaps gravitation. That may give us a clue some day, but at present we have not been able to detect its speed of transmission at all. No plan has been devised for measuring it. Nothing short of the creation or destruction of matter seems likely to serve; creation or destruction of the gravitational unit, whether it be an atom or an electron or whatever it is. Most likely the unit of weight is an electron, just as the unit of mass is.

OLIVER LODGE

(To be concluded)

A SUMMARY OF THE WORK OF THE U. S. FISHERIES MARINE BIOLOGICAL STATION AT BEAUFORT, N. C., DURING 1912

THE laboratory of the Bureau of Fisheries at Beaufort, North Carolina, was open as usual during the summer of 1912, and opened about the middle of June, 1913, to investiga-

tors engaged in the scientific and economic problems of the Bureau and to independent workers. Following is a brief summary of the work of the station and some of the results attained during the year 1912.

The laboratory continued its cooperation with the U. S. Weather Bureau, keeping a daily record of the maximum and minimum temperatures, precipitation (rain and melted snow), etc. These data were forwarded monthly to the Raleigh office.

Greatly needed improvements to grounds and buildings were begun during the year. The library was removed from the crowded laboratory to new quarters on the museum floor, and its contents are being arranged and catalogued according to the system used in the Washington office. A large number of state and other reports were received during the year, scientific works adapted to the needs of the station have been ordered, and an attempt is being made to assemble all publications relating to the fauna and flora of the region. All investigators who have or are publishing such papers are urged to forward separates to the library.

The cultural experiments with the diamond-back terrapin were continued with marked success, and the feasibility of terrapin culture on a commercial basis is practically assured. The 1912 brood numbered over 1,220, more than three times as many as in 1911, and indications are that for the stock of adults on hand the maximum has not been reached. This brood, with those of 1911, 1910 and 1909, makes a total of over 1,900 young terrapin hatched in the enclosures at the laboratory. In the fall of 1911 and spring of 1912, 66 adult terrapin from Texas were purchased, and from the eggs laid by these a sufficient number of young were obtained to begin the experimental work with this form. Professor W. P. Hay had general supervision of much of this work.

It is the purpose of the laboratory to collect all possible data bearing on the fishes of the South Atlantic region, to conduct fish-cultural experiments to show the feasibility of increasing the annual yield by artificial propagation, and to add to knowledge of the life-histories of as many forms as possible. As a basis for this work, the compilation of all existing information on the local fishes has been undertaken. A card catalogue of species and a systematic list with synonymy of published references for the region are practically completed, about 235 species being represented.

In an examination of old collections in the laboratory several examples of a mad-tom, *Schilbeodes gyrinus* (Mitchill), from Lake Mattamuskeet, N. C., were found. This is believed to be the first record south of the Potomac River at Washington, D. C. Two additional species not previously reported from North Carolina were taken during the summer. The first, a galeid-shark, *Hypoprion brevirostris* Poey, represented by two examples, had been reported as far north as Charleston, S. C.; one specimen was 7½ feet long, the largest recorded. A southern sting-ray, *Dasyatis sabina* (Le Sueur), was also taken. This species appears to be quite abundant and has probably been confused heretofore with small examples of some of the other species. An example of *Carcharhinus acronotus* (Poey), the second record for the coast, was also obtained. A fine example of the interesting ray *Mobula olfersi* (Muller & Henle) was presented to the laboratory by Mr. Russell J. Coles.

On July 26, 1912, a beaked whale (*Mesoplodon*) 16 feet long was stranded on Bird Island Shoal in the harbor. The head, tail and one of the pectoral fins were sent to the U. S. National Museum, where Dr. F. W. True found it to be an undescribed species and has since given to it the name *M. mirum*.[1]

The investigators and independent workers have furnished the data on which the following brief summary of their work is based:

Professor W. P. Hay who, during July, August and September, continued his work on the propagation of the diamond-back terrapin, also spent considerable time on the study of the crustacean fauna of the Beaufort region, and began a series of experiments on the artificial propagation of the loggerhead turtle.

[1] *Smithsonian Misc. Coll.*, Vol. 60, No. 25, March 14, 1913.

Early in July a nest of the loggerhead turtle, containing 135 eggs, was found on the ocean beach of Bogue Bank. The eggs were removed to the laboratory and placed in hatching boxes, and 75 young turtles were hatched and retained until winter. The economic value of the loggerhead turtle is at present very small, but the data secured from the experiments at the laboratory will doubtless be useful if an effort is ever made to cultivate more valuable species of sea turtles.

The decapod crustaceans of the Beaufort region were studied some years ago by Dr. H. A. Shore, but pressure of other matters made it impossible for him to complete his report. It is this unfinished work that has been taken up by Professor Hay and is being put in shape for publication.

Dr. H. S. Davis, of the University of Florida, devoted his time largely to studying the life-history of a dimorphic species of *Myxosporidia* occurring in the urinary bladder and ureters of the squeteague, *Cynoscion regalis*. This species occurs in two very different forms (one disporous, the other polysporous) and possesses many characters of great interest, notably a method of reproduction by internal budding hitherto unknown in the *Myxosporidia*. The development of the spores was worked out in detail and has been found to differ in many respects from the published accounts of spore formation in other species. The account of this work will shortly be ready for publication. Observations were made on a number of species of *Myxosporidia* occurring in the gall bladders of sharks and others inhabiting other marine fishes, and a considerable amount of material was preserved for future study.

Dr. J. F. Abbott, of Washington University, St. Louis, Mo., conducted various experiments on the fiddler crab (*Uca*), which abounds in the neighborhood of Beaufort Harbor.

(*a*) The question of the relative permeability of tissues and particularly of gill membranes to pure distilled water is still an open one. *Fundulus heteroclitus* appears to be impermeable to and unaffected by immersion in pure distilled water. From the apparent immunity of the fiddler crab to fresh and distilled water it appears at first that it, like *Fundulus*, offers a similar exception to the rule that animal membranes are freely permeable. It was discovered after prolonged experiment that the crab stores up very small quantities of sea water in its gill chamber, with which it modifies the pure water sufficiently to preserve its life. If the gill chamber be cut away and the cavity washed out, this immunity disappears and the crab succumbs to the effect of the water with an increase of weight (indicating the penetration of water) and a loss of salts (discoverable by titrating the immersing medium for chlorides). If the amount of water be small the crab is able by emitting minute quantities of electrolytes to alter the medium sufficiently to nullify the destructive solvent action of the pure water on the gill-membranes. An account of this portion of the work has been published in the *Biological Bulletin* of the Marine Biological Laboratory at Woods Hole, Mass. (Vol. 24, p. 169, 1912).

(*b*) Other lines of experiment on the nullifying action of one poisonous component of the sea water by another were carried out, leading to results which in general substantiate J. Loeb's hypothesis of balanced solutions as worked out on marine vertebrates.

(*c*) In connection with the storage of water in the gill-chamber mentioned above, the morphology of the apparatus by means of which the crab is enabled to leave the water for long intervals of time was worked out. An opening is to be found between the third and fourth pereipods, which is fringed with hairs and leads up through a narrow channel to a space above the gills. It is provided with a valvular stop and with a structure which appears to function as a sense organ. It was ascertained that the crab does not "breathe air" as frequently stated, but aerates the water thus retained in its gill chambers.

(*d*) During the summer of 1912 a large number of fiddler crabs were captured and preserved for the purpose of determining the variation constants and the establishment of "place modes." It is planned to continue the work for a number of seasons in order to de-

termine if possible what effect climatic and environmental factors may have on the variability of the species. The now completed laborious task of measuring (involving over 10,000 measurements under a magnifying glass) has been carried out in the laboratory of the department of zoology of Washington University.

Dr. Abbott also made studies of the blood of *Thallasema,* an echiurid worm that inhabits the dead tests of the "sand dollar." This fluid is interesting from the standpoint of its corpuscles, which, like those of vertebrates, are of two kinds—ameboid forms and hemoglobin-bearing, respiratory cells. The individual cycle of these cells and their probable functions were worked out during the latter part of the summer, and the results are in press in the *Washington University Quarterly.* In about twenty-five per cent. of the worms studied the hemoglobin-bearing corpuscles formed were found to be parasitized by an undescribed species of *Hæmogregarina*—the first record of a hæmosporidian parasite in an invertebrate host. Portions of the life cycle of the form were worked out, and it is hoped to complete this at some future time.

Mr. L. F. Shackell, instructor in pharmacology, St. Louis, University School of Medicine, was engaged in a study of methods for protecting wood against the attacks of marine borers. Nearly seventy pieces of wood were coated with mixtures containing a variety of poisons, and are being allowed to remain in the water of Beaufort Harbor for nine months, the last three of which will coincide with the breeding season of *Teredo* and *Limnoria.*

Professor H. V. Wilson, of the University of North Carolina, spent a part of the summer in an investigation bearing upon the question of the reciprocal interaction of cells of different species, his observations dealing especially with the behavior of the amœbocytes in the lymph of the sea urchins *Arbacia* and *Toxopneustes.*

Dr. James J. Wolfe, professor of biology in Trinity College, Durham, N. C., spent seven weeks at the laboratory completing his investigation of *Padina,* begun here in the summer of 1910, so far as the work which had to be done at the seaside is concerned. Forty-eight cultures of eggs and tetraspores were started in aquaria in the laboratory and later transferred to various localities in the harbor. These were collected on a special trip made to Beaufort, September 25. A subsequent examination, not yet quite complete, shows fairly conclusively an alternation of a sexual with an asexual generation. From July 18 to September 1 general records were kept covering rate of growth, formation of hairs, and periodicity in the production of sex organs. The foregoing, together with a cytological examination at critical stages, is now being incorporated in a paper on "The Life History of *Padina.*"

Dr. A. J. Goldfarb, of the College of the City of New York, visited the laboratory from August 6–17 in order to continue certain experiments begun earlier in the season at the Marine Biological Laboratory of the Carnegie Institution, on the grafting of eggs together and on certain changes produced by chemical means. Extensive dredging operations about the harbor and close to the laboratory appear to have polluted the harbor waters, and it was found necessary to bring in sea water from outside the harbor to secure normal development of the fertilized eggs of *Toxopneustes variegatus* into perfect plutei larvæ. With this water the eggs when subjected to the action of a $\frac{5}{8}$ *M* NaCl solution tended to fuse together in large numbers, and to continue their fusion into various types of single and double organisms. These fusions were produced in the same manner and gave rise to the same types of fusions as those obtained at the Tortugas earlier in the season, and established beyond all question that this new method for the production of fused eggs and larvæ is superior, in simplicity, in absence of disturbing physical factors, and in the number of fusions, to the methods formerly used by the writer, by Driesch and by Herbst.

Dr. Albert Kunz, of the University of Iowa, studied the habits, the morphology of the reproductive organs and the embryology of the viviparous fish, *Gambusia affinis,* and the early

developmental stages of two species of teleosts whose eggs were found in the plankton.

Gambusia affinis is exceedingly abundant in the vicinity of Beaufort in all the freshwater streams entering the harbor and in the shallow brackish waters. This species is of economic importance as a destroyer of insects and insect larvæ. Wherever it inhabits waters in which mosquitoes breed, the mosquito larvæ constitute its principal food. The introduction of these fishes into the natural waters as well as into artificial ponds, aquatic gardens, etc., in mosquito-infested regions, may play an important rôle in the extermination of these pests.

One of the most interesting points studied by Dr. Kunz was the structure of the apparatus controlling the modified anal fin in the male *Gambusia*. This fin functions as an intromittent organ and is controlled by a powerful muscle which has its origin on a bony process projecting ventrally from the fourth to the last abdominal vertebræ and the modified anal spines of the first three caudal vertebræ and is inserted on the proximal end of the anal fin rays. The third, fourth and fifth rays of the fin are enlarged, greatly elongated and variously curved, bearing short spines on their distal portions. The interhemal which articulates with the third ray is enlarged and sufficiently elongated to articulate with the two anterior processes, on which the muscle controlling the anal fin has its origin. The fifth ray may be drawn forward at one side of the fourth and brought into proximity with the third. In this manner a groove or tube is formed through which the milt is transferred from the male to the female. The results of this work are to be published in the near future.

On August 3, 1912, pelagic eggs of the two species of teleosts were taken in the tow-net. Both are spherical in form and comparatively small, having a diameter of .6 to .7 mm. One kind, probably those of *Trichiurus lepturus*, are almost perfectly transparent and contain no oil-globule. The other, perhaps those of an engraulid, contain an oil-globule and numerous minute pigment spots. Eggs taken at the same hour on successive days were found to be in approximately the same stage of development. Spawning obviously occurs in the evening, probably between five and eight o'clock. Before six o'clock in the morning the embryo is well differentiated, and at 36 hours after spawning the little fishes are already hatched. Observations on the development of these two species are still incomplete. It is expected that these studies will be extended and the species positively identified.

Following the work of Thompson, Johnson, Tims and Dahl on the scales of the salmon and English brook trout, with special reference to age determinations and life-history indications, Mr. H. F. Taylor, of Trinity College, Durham, N. C., undertook to verify and amplify their conclusions by investigating the scales of an important American food fish, *Cynoscion regalis* being chosen.

Age may be determined with more or less accuracy by enumerating the annuli or supposed zones of growth. Various methods of bringing out these annuli clearly by stains, polarized light, etc., were employed. The results will be explained in a paper to be published shortly.

The evidences found by Mr. Taylor do not warrant the assumption that annuli are due to retarded growth, as was hitherto supposed, but they must be due to other causes which are at present somewhat doubtful. At all events it is fairly certain that if these fishes grow more slowly in winter than in summer there is no evidence of this on the scales. Distances between the annuli are found to represent, proportionately, the length of the fish at the times of the formation of the several annuli.

The nature of the radii was also studied. They were found not to be constant, but to vary with the activity of the fish and with the part of the body from which the scale was taken. The evidence indicates that they are hinges through the superior calcified layer to permit the scale to bend in adaptation to the motion of the body of the fish. On the head, etc., where there is no flexibility, there are no radii on the scales; and their number on scales

from other parts agrees with the shape, size and thickness of the scale and the motion of the part. If this conclusion stands it will seriously modify systems of classification employing radii as characters.

Messrs. William J. Crozier and Selig Hecht, of the College of the City of New York, who were assigned to the director for duty, accompanied the various collection trips, made extensive collections of fishes and kept a complete record of all observations, devoting special attention to those relating to the food, habits, rate of growth, relative abundance and distribution of the fishes taken. They also studied correlations among weight, length and other body measurements of the squeteague (*Cynoscion regalis*). The coefficient of correlation of weight and length and the constant, which if multiplied by the cube of the length gives the weight of the fish, were determined. Stomach contents of a large number of examples of this species were examined. The results indicate that the relative proportions of the forms of life commonly eaten depend upon the size of the fish and that the food varies with the locality.

Lewis Radcliffe,
Director

SCIENTIFIC NOTES AND NEWS

Professor William Bateson, director of the John Innes Horticultural Institution, has been elected president of the British Association for the Advancement of Science for the meeting which will be held next year in Australia.

On the occasion of the meeting of the International Geological Congress at Toronto, the University of Toronto conferred the degree of doctor of laws on the following geologists: T. C. Chamberlin, U. S. A.; W. G. Miller, Canada; P. M. Termier, France; R. Beck, Germany; J. J. Sederholm, Finland; T. Tschermyschev, Russia, and A. Strahan, England.

Professor Lillien J. Martin, professor of psychology at Stanford University, has had the honorary degree of doctor of philosophy conferred upon her by the University of Bonn.

Professor Bier and Professor Körte, of Berlin, have been named as honorary members of the Royal College of Surgeons in London.

According to a note in *The Observatory* the American astronomers present at the meeting of the Solar Union at Bonn were: Campbell, St. John and Burns, from California; Stebbins, from Illinois; Parkhurst, Slocum and Gingrich, from Yerkes; Schlesinger, from Allegheny; Russell and Shapley, from Princeton; Ames, from Baltimore; Doolittle, from Philadelphia; Nichols, from Cornell; Pickering, Bailey, Miss Cannon and Mrs. Hastings, from Harvard; Miss Whiting and Miss Allen, from Wellesley, and Plaskett, from Ottawa.

Dr. Carl Correns, professor of botany at Munster, has been appointed director of the Research Institute for Biology of the Kaiser Wilhelm Society. Dr. Spemann, professor of zoology at Rostock, has been appointed assistant director.

Prince Galitzin has become director of the Observatoire Physique Central Nicolas, St. Petersburg.

Mr. Aksel S. Steen has been appointed director of the Meteorological Institute of Norway, in succession to Dr. H. Mohn, who has retired.

Mr. C. A. McLendon, for the past five years botanist and plant-pathologist to the Georgia Experiment Station, in charge of plant-breeding investigations, has tendered his resignation to take effect October the first, after which date he expects to be engaged in private business.

L. F. Hawley, Ph.D. (Cornell), formerly in charge of the section of wood distillation and chemistry of the U. S. Forest Service, is now the director of a forest products department recently established by Arthur D. Little, Incorporated, Boston, Mass.

Dr. Calvert M. DeForest has been appointed deputy health officer of the Port of New York. Dr. DeForest has recently returned from Libau, Russia, where he has been in the Public Health Service for the last five years.

FREDERICK G. CLAPP, of the Associated Geological Engineers, has returned from the gas fields of Hungary, and has gone to New Brunswick in company with Mr. Myron L. Fuller and Mr. Lloyd B. Smith of the same bureau.

MR. D. A. BANNERMAN has returned from a zoological mission to the eastern islands of the Canary group, undertaken with the object of procuring birds for the Natural History Museum, London.

SIR WILLIAM OSLER will distribute the prizes and deliver an address at St. George's Hospital on October 1.

THE lectures at the Harvey Society in the Academy of Medicine, New York City, will be inaugurated on October 4 by a demonstrative lecture by Dr. A. D. Waller, of London, entitled "A Short Account of the Origin and Scope of Electrocardiography." Subsequent lecturers are Professor Adolph Schmidt, Halle; Dr. Charles V. Chapin, Providence, R. I.; Dr. Rufus Cole, Rockefeller Institute; Professor G. H. Parker, Harvard; Dr. Victor C. Vaughan, Ann Arbor, Mich.; Professor Sven Hedin, Upsala, Sweden, and Professor J. J. R. Macleod, Western Reserve University.

A LECTURE will be delivered on October 7 at the University of Birmingham by Professor Arthur Keith, F.R.S., on "The Present Problems Relating to the Antiquity of Man."

A TABLET has been unveiled at Primiero, Southern Tyrol, on the house in which Alois Negrelli was born, to commemorate his work as surveyor of the Suez Canal.

MR. EDWARD LYMAN MORRIS, since 1907 curator of natural science in the Museum of the Brooklyn Institute and since 1898 special plant expert of the U. S. National Museum and the U. S. Department of Agriculture, died on September 14, aged forty-three years.

PROFESSOR MOSES CRAIG, formerly professor of botany at the Oregon Agricultural College and botanist of the station, later in charge of the herbarium of the Shaw Botanical Garden, St. Louis, died on August 31. He was graduated from the Ohio State University in 1889 and received a master's degree from Cornell University in 1890.

WE learn from the London *Times* that the future of the educational museums, founded and equipped by the late Sir Jonathan Hutchinson, at Haslemere, Selby (Yorks), and 22, Chenies-Street, London, is causing some concern. In his will Sir Jonathan leaves the museums to his trustees to dispose of as they may think best. In his lifetime he spent on the museums and their equipment at least £30,000. At Haslemere there is a strong feeling that everything should be done to retain the museum for the town, and it is understood that the family are willing to hand it over to a responsible committee or body of trustees so that the museum may be placed on a permanent and public basis. The annual cost of maintenance on present lines is about £400, and an appeal will shortly be issued with the hope of securing this sum for five years at least, it being thought that by that time those who are interested in the matter will have had an opportunity of deciding what are the best steps to be taken for the permanent control and maintenance of the museum.

UNIVERSITY AND EDUCATIONAL NEWS

BY the will of Miss Katherine Allen, of Worcester, the Worcester Polytechnic Institute received a bequest amounting to about $100,000.

MRS. RUSSELL SAGE has given $34,000 to Syracuse University, of which $30,000 is for the Joseph Slocum Agricultural College.

MRS. ELLA STRONG DENISON, widow of the late Dr. Charles Denison, proposes to give a medical building to the University of Colorado. The wings will be used for laboratories, and the central tower will have a lecture room and a library. The west wing is now being built. It will be called the Henry S. Denison Laboratory in memory of Mrs. Denison's son, who was a member of the University of Colorado faculty.

FROM the list of doctorates conferred by American universities, published in SCIENCE

for September, there were omitted four degrees given by the University of California: The recipients were: Harold Childs Bryant, in zoology; Wilson Gee, in zoology; Harry Noble Wright, in mathematics, and Friedrich Alexander Wyneken, in German.

Dr. A. G. Pohlman, of Indiana University, has accepted the professorship of anatomy in the school of medicine in St. Louis University.

Dr. C. L. Andrews, of the Johns Hopkins University, has been appointed professor of anatomy in the University of Mississippi Medical College.

Dr. Leonard W. Ely, of Denver, Colorado, was appointed associate professor of orthopedic surgery, and Dr. Ralph W. Majors, instructor in pathology, at a recent meeting of the board of trustees of Stanford University. On October first there will be opened a clinic in orthopedic surgery in quarters which are being fitted up for the purpose in the medical school buildings in San Francisco.

Ralph W. Curtis, B.S.A., who was for four years assistant superintendent of the Arnold Arboretum of Harvard University, has been appointed assistant professor of landscape art in the college of agriculture of Cornell University.

Josiah Main, of the State Normal School, Hays, Kansas, has been appointed professor of agriculture for schools in the State Agricultural and Mechanical College, at Stillwater, Oklahoma.

Paul S. Welch, Ph.D. (Illinois), has been appointed instructor in entomology in the Kansas State Agricultural College, and assistant entomologist of the experiment station to fill the place made vacant by the resignation of Dr. M. C. Tanquarry, who is accompanying the Crocker Land Arctic Expedition. Dr. John W. Scott, assistant professor of zoology and assistant station zoologist in charge of investigations in parasitology in the Kansas State Agricultural College, has resigned to become professor and head of the department of zoology and parasitology in the University of Wyoming. J. E. Ackert, Ph.D. (Illinois), has been appointed to the position in Kansas made vacant by Dr. Scott's resignation.

The following appointments to the faculty of the Alabama Polytechnic Institute and Experiment Station have just been made: Professor Ernest Walker, graduate of Cornell, formerly the head of the department of horticulture in the University of Arkansas, to be head of the department of horticulture; G. S. Templeton, B.S. (Missouri, '11), who has been for the past two years instructor in animal industry in the Texas College, to be head of the department of animal industry; L. S. Blake, a graduate of the University of Michigan, becomes acting head of the department of pharmacy as substitute for Professor E. R. Miller, who becomes acting assistant professor of plant chemistry in the University of Wisconsin. Lucius W. Summers, who has been assistant professor of animal industry for the past two years, has resigned to accept the position of professor of animal industry in the Virginia Polytechnic Institute.

Dr. J. Austin Bancroft has been appointed by the governors of McGill University Dawson professor of geology.

Dr. A. D. Imms has been appointed to the newly created post of reader in agricultural entomology at the University of Manchester. Dr. Imms was formerly professor of biology in the University of Allahabad, and afterwards forest entomologist to the government of India at the Imperial Research Institute, Dehra Dun.

DISCUSSION AND CORRESPONDENCE

THE DATA OF INTER-VARIETAL AND INTER-SPECIFIC COMPETITION IN THEIR RELATION TO THE PROBLEM OF NATURAL SELECTION

To the Editor of Science: Biometricians have clearly demonstrated[1] that of the variations which occur within the limits of the species some have far less chance of survival than others. In short, the intra-specific death

[1] See several papers in *Biometrika* and two general reviews in the *Popular Science Monthly* for 1911 and 1913.

rate is selective. The difficulties, however, of the problem of natural selection, the least investigated of all of the primary factors of organic evolution, demand the collection of evidences from every possible source.

The purpose of this letter is to call attention to inter-varietal and inter-specific competition as a source of information on natural selection, to illustrate the point by one or two recently published observations, and to urge the accumulation of more (and more precise) data of this kind by those field naturalists and experimentalists who have the opportunity for this sort of work.

The kind of studies to which I refer are illustrated by Brimley's interesting account[2] of the capture of Raleigh, N. C., by the wharf rat, *Mus norvegicus*. Up to 1909, the roof rat, *M. alexandrinus*, was the only species seen during a residence of twenty-five years. Since then it has been nearly or entirely replaced by the wharf rat.[3]

For a second illustration turn to botanical material.

Varieties of plants are generally believed to differ in their susceptibility to disease. An interesting demonstration of this is furnished by researches on the potato fungus, *Phytophthora infestans*. Jones, Giddings and Lutman have shown[4] that there is a correlation between the percentage growth of the fungus on various varieties in the test-tube and the percentage of rot observed in field trials on clay and sandy soil by Stuart. They find for laboratory growth and loss on clay a correlation of $r = .584 \pm .059$ and for laboratory and sandy soil a correlation of $r = .594 \pm .055$.[5] For convenient comparison I have also worked out the correlation between the percentage rot of the same varieties on clay and sandy soil, using Stuart's data as quoted by Jones and his associates. For the ungrouped material $r = .707 \pm .045$.

Clearly enough there is a pronounced individuality in the varieties with respect to their capacity for resisting disease. The interest of such a result from the standpoint of natural selection is clear, for in free competition more susceptible strains would be rapidly weeded out, and the morphological or physiological characteristics to which their inferiority is due would be lost.

Now of course in neither of these cases do we know why (*i. e.*, because of what peculiar characteristics) one variety or species was less capable than another of survival. Nor can we know until the questions are more intensively studied. But one can not doubt that these problems will yield to proper and persistent observation.

My point is merely that this sort of work may (if carried out extensively and intensively enough) have a most important bearing upon the two fundamental questions of natural selection. First, is the death rate random or selective? Second, if selective, what weight has each individual character in determining the chances of survival of the individual? In seeking the answer to the second question it may be desirable to deal with characteristics as strongly contrasted as possible—with varietal or specific differences instead of with intra-varietal variations—in order that the proximate causes of the differential mortality may be more easily recognized.

The value of such work for the problem of natural selection will be quite supplementary to that for which it was primarily carried out. May we not, therefore, have more observations of this kind, carried out in such detail that they may be of value to the evolutionist seeking to ascertain the selective value of individual characters?

J. ARTHUR HARRIS

STATION FOR EXPERIMENTAL EVOLUTION,
August 20, 1913

[2] Brimley, C. S., "Capture of Raleigh by the Wharf Rat," *Journ. Elisha Mitchell Sci. Soc.*, 28: 91–94, 1912.

[3] Dr. Hatai tells me that when he placed white rats on an island inhabited by the brown rat, *M. norvegicus*, the two forms at once established different centers and began fighting each other. See also *Year Book Carn. Inst. Wash.*, 10: 83–84, 1912.

[4] Jones, L. R., N. J. Giddings and B. F. Lutman, *Bull. Vt. Agr. Exp. Sta.*, 168: 74–81, 1912.

[5] These are calculated on grouped data. I have recalculated without classing and find results agreeing within less than half the probable error.

PREPOTENCY IN AIREDALE TERRIERS

I HAVE recently had occasion to make a careful analysis of the ancestry and get of Airedale terriers. In view of the fact that this variety of dogs was manufactured only some fifty years ago out of known materials[1] and now breeds true to type the results are interesting, especially when compared with Davenport's studies in trotting-horse pedigrees.[2]

The records of the English and American Kennel Clubs' Stud Books show that to January, 1913, 80 dogs and 69 bitches have won their championship in both countries. Since to become a champion, a dog must receive a certain number of awards under at least three different judges, it is safe to assume that winners of the title are above the average of the variety. Certainly to breed a champion is the object of dog fanciers' breeding experiments.

Of the 80 dog champions, 39 were sired by champions. Of the 80 champions 38 had one grand-sire a champion, and 23 champions had both grand-sires champions. Just one less than half of all dog champions were sired by a champion, and about three fourths had either one or both grand-sires champions.

Of the 80 dog champions, however, 53 never sired a champion of either sex. Only 27 of the dog champions produced championship winners. Of these 27 sires of champions, but 13 produced more than one champion. However, these 13 exceptional sires produced 49 of the 149 Airedale champions; almost a third of all the champions of both sexes.

In the second generation, sons of champions sired 47 dog and 38 bitch champions, and daughters of champions were the dams of 43 dog and 22 bitch champions. It should be noted that champions both of whose grand-sires were champions get into these figures twice, as both the get of a champion's son and also of a champion's daughter. Of the 80 dog champions, 17 are bred this way.

Of the 80 dog champions, however, only 24 actually appear as grand-sires of champions, and but 10 are the grand-sire of 4 or more champions. Even more striking evidence of the prepotency of certain dogs as producers of champions is that those dogs who sired 2 or more champions almost invariably appear among those whose sons and daughters have produced more than 4 champions. The exceptional sires are also the exceptional grand-sires. The following table shows the champions in the ancestry and get of these exceptional breeding individuals.

Champion Ancestry					Champion Get					
					Sire of		Sire's Sire		Dam's Sire	
No.	Sire	Dam	Grand Sires	Grand Dams	♂	♀	♂	♀	♂	♀
5			1	1	2	2	1			1
9							2	1	2	
17	1		1		5	3	8	4	6	6
21			2		5	1	7	5	1	2
22	1		1			2	1		1	
28					1	2	1			
44			1		4	1	12	10	7	2
45			2		1	1	5	3		3
47	1		1		1		2	1	3	1
53	1	1	1		2	1				
56	1		1				2	1	1	
58					2	1	1	2	3	1
59	1		1		4					
65	1		2			2		3	1	
66	1				1	2				
72	1				2	2	3	5	2	2
16	9	1	8/3	1	30	20	45	35	27	18

These 16 champions have sired 50 champions, or, in other words, a third of all the Airedale champions have been sired by something less than a third of the dog champions.

Moreover, a glance at the pedigrees of these 16 phenomenal producers show them all to be more or less closely related. All trace back to Cholmondeley Briar (No. 9 in the above table). The three greatest producers of the lot are Master Briar (44), Clonmel Monarch (17), and Crompton Oorang (21). Master Briar is a grand-son of Cholmondeley Briar and the sire of Clonmel Monarch. Crompton Oorang is by a son of Master Briar out of a daughter of Clonmel Monarch. Without tracing out all the relationships in the dogs of the table, it may be said that the two living dogs (21 and

[1] Buckley, "The Airedale Terrier," 1907; Haynes, "The Airedale," 1911; Palmer, "All about Airedales," 1912.

[2] "Principles of Breeding," pp. 551–567.

59) are Crompton Oorang and his son, Rockley Oorang.

Practically all show dogs are placed at public stud, and any champion, thanks to the advertising his winnings give him, will be popular. The 53 champions who never sired a champion can not therefore be excused on the plea of lack of opportunity. They would certainly receive more bitches than a non-champion, unless this dog had made a great reputation as a sire.

The full table, showing the ancestry and get of all Airedale champions, and a similar one for Scottish terriers will be published in my forthcoming book on dog breeding.

WILLIAMS HAYNES

MITOSIS IN THE ADULT NERVE CELLS OF THE COLORADO BEETLE

IN a recent study of the development of the nerve cells through larval, pupal and adult stages in the honey bee, we had ample opportunity to note the method of division and growth. After the very early larval stages there is formed a regular mitotic figure in each multiplying nerve cell. These division figures are not equally abundant in all our material, which may account for the assumption that there is a rhythm in the normal growth of nerve cells. Mitosis does not stop at the end of the larval period, but continues for a time in the pupal stage. We have observed perfect mitotic figures in bees in the early pupal stages of metamorphosis. These figures are exactly like those occurring in the larval stages.

The larval life of the honey bee is relatively inactive, which affords an interesting contrast with the active existence of the common potato beetle. The results of this comparison will appear in a separate paper. While making the comparative study of the larval as well as pupal and adult stages in the growth of the nerve cells we noted in some of the adult material unmistakable evidence of nerve cell division. Close examination showed that there were many nerve cells in one animal dividing in the normal mitotic manner. Centrosomes, spindle fibers and astral rays were all complete. The chromosomes were too compactly massed to be counted. In one field of the 2 mm. oil immersion objective we found six cells undergoing division. Others appeared in other parts of the ganglionic mass.

Our study upon the growth of the nerve cells in the honey bee and the potato beetle indicate that we may expect to find nerve cells regularly dividing by mitosis through the pupal and into adult life.

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SCIENTIFIC BOOKS

Sigma Xi Quarter Century Record and History 1886–1911. Compiled by HENRY BALDWIN WARD, Secretary of the Society of the Sigma Xi, with the assistance of the Chapter secretaries. University of Illinois. Urbana-Champaign. Pp. xii + 542.

A brief statement of the society whose achievements for a quarter of a century are given in the octavo volume which has just been published under the above title will perhaps best describe its importance.

In the early spring of 1886 the feeling that students of science who were not eligible to election in the well-known honor college fraternity, Phi Beta Kappa,[1] should organize a similar honor society to which those worthy followers of Agassiz, Darwin and Haeckel should be admitted was clearly recognized at more than one college, and especially at those universities where science was made an important feature of the curriculum.[2]

Accordingly, at Cornell University in November, 1886, the society of the Sigma Xi was

[1] Organized in 1776 at William and Mary College in Virginia.

[2] Let me call attention at this point to the fact that very early in the history of the School of Mines of Columbia University in New York those students who were able to enter the senior class without conditions were given the privilege of wearing the badge of crossed hammers in the course of mining engineering, and of the Liebig's potash bulbs in the chemical course.

organized. It takes its name from the initial letters of two Greek words signifying "Companions in Zealous Research." The object of the organization, as given in its constitution, is to encourage original investigation in science, pure and applied, by meeting for the discussion of scientific subjects; by the publication of such scientific matter as may be desirable; by establishing fraternal relations among investigators in the scientific centers; and by granting the privilege of membership to such students as during their college course have given special promise of future achievement.

Membership in this society is of three kinds: active, alumni and honorary. Naturally the first class is the most important and includes, as a rule, professors, instructors, graduates and such undergraduates as may be found worthy. The undergraduates are usually chosen in the senior year, following in this respect the custom of Phi Beta Kappa, although in some institutions, as, for instance, the University of Chicago, it has been the policy to admit only graduate students to membership. The alumni members are chosen from graduates of at least five years' standing, who have demonstrated their right of membership by investigation, while honorary members may be selected from those who have achieved eminence as scientific workers, although as yet none such have been elected.

From the beginning it was evident that the society would succeed. Chapters were organized at Rensselaer and Union in 1887, at Kansas in 1890, and at Yale in 1895. In addition to the foregoing there are now chapters at Minnesota (1896), Nebraska (1897), Ohio (1898), Pennsylvania (1899), Brown (1900), Iowa (1900), Stanford (1901), California (1902), Columbia (1902), Chicago (1903), Michigan (1903), Illinois (1903), Case (1904), Indiana (1904), Missouri (1905), Colorado (1905), Northwestern (1906), Syracuse (1906), Wisconsin (1907), Washington (1907), Worcester (1908), Purdue (1909) and Washington, St. Louis (1910).

The membership in 1886 was but 14, but it has grown steadily and persistently ever since; for in 1891 it was 267, in 1901, 1,559, and in 1911, 7,498, which number is annually increased by between 600 and 700 (659 in 1911) new members, of which in 1911 324 were undergraduates.

Annual conventions are held on the Tuesday evening of the week of the meeting of the American Association for the Advancement of Science, at which time the policy of the society comes up for discussion and such other public business as may be desired. Delegates from the chapters, together with the general officers, are members of the Council.

It is not easy to review the achievements of Sigma Xi during its existence of a little more than a quarter of a century. This difficulty lies in knowing just what to say. There is no danger of saying too much, but there is decided danger in saying too little. Its mission is to encourage science and to foster original investigation.

Science has been distinctly advanced by the popular public lectures and addresses made before many of the chapters by such eminent authorities as Charles F. Chandler, R. H. Chittenden, George W. Goethals, G. E. Hale, L. O. Howard, David Starr Jordan, A. A. Michelson, C. S. Minot, E. W. Morley, E. L. Nichols, C. R. Van Hise, Arthur G. Webster, Harvey W. Wiley and many others.

In the celebrations of the centenary of Darwin's birth, it took an active part, and important commemorative meetings with appropriate addresses were held when the bicentenary of Franklin's birth occurred.

Of far-reaching importance was the investigation by the California chapter of the condition actually existing in the region about San Francisco concerning the bubonic plague and the results of the report were most potent at a time when the existence of that frightful disease on the Pacific coast was disputed.

Not the least of its valuable contributions is the fact that it has brought about an increased interest in Phi Beta Kappa. It affiliates agreeably with its older rival at Columbia, Kansas, Minnesota and Pennsylvania, alternating addresses at commencement at certain of these universities, and holding joint meetings at others. The existence of Tau Beta Pi, the honor fraternity in institu-

tions of applied science, is, I am sure, very largely due to the success of Sigma Xi.

Sigma Xi stands "for intellectual energy rather than sordid ambition," and the volume so ably compiled by Professor Ward richly demonstrates the fact that it "has become a prominent factor in most of our universities." In the words of one of its founders in consequence of its influence:

> Men have come to know that knowledge of the present is far more important than tradition—that individual discernment, power of initiative, and honesty, surpass all authority in the equipment of a scholar of the new sort.

MARCUS BENJAMIN

An Introduction to the Chemistry of Plant Products. By PAUL HAAS and T. G. HILL. Published by Longmans, Green and Co., London, New York, Bombay and Calcutta. 1913. Pp. xii + 401.

The progress of chemistry, perhaps more than of any other science, may be divided into great epochs, in each of which one branch of the science is found to be far more productive of permanent results than are the other divisions.

The centuries-long period of alchemy gradually merged into the period when chemical researches were conducted with the view of enlarging the number of compounds which could be utilized in medicine.

Following the discovery of the nature of combustion, we begin to find the first organized chemical research, devoted in the main to inorganic chemistry, which rewarded us with a gradually increasing number of elements, with the atomic hypothesis, and the gas laws.

Thus until 1828 nearly all of the chemical investigations were confined to inorganic chemistry, for the compounds of carbon were supposed to be formed only by the action of life. When, however, Wöhler made his famous synthesis of urea, a new field was opened and the immense number of organic compounds listed in "Beilstein" are in a large measure the result of the studies of the period of organic chemistry.

For a time organic chemistry overshadowed inorganic chemistry until, under the leadership of men like Arrhenius, Ostwald, Nernst and Van't Hoff, a new chemistry was created which we know as physical chemistry. And even in our own time we have seen the science of radioactivity follow the discovery of radium by Mme. Curie.

During all of these advances the chemistry of the life processes has been more or less neglected. To be sure, a great many of our universities list courses in "physiological chemistry," but until very recently these have been devoted almost entirely to the study of nutrition and the chemistry of pathology, and even to-day the study of the chemistry of the life processes is only at a beginning. This is perhaps necessary, for it would be a useless task to undertake to determine and measure the life processes without the exact knowledge furnished by the organic and physical chemists.

We are thus, probably, near the beginning of a period of biological chemistry, not only the chemistry of animal life, but the chemistry of plant processes as well, not only from the standpoint of the physician and utilitarian, but from the broader standpoint of the study of life itself, its chemical products and the laws by which it is governed.

We have many admirable text-books dealing with physiological chemistry, but text-books which are suitable for a course in plant chemistry are rare. This may perhaps in part explain the absence of such courses from the curricula of our universities. It is, therefore, a pleasure to find such a book as "An Introduction to the Chemistry of Plant Products."

Modeled somewhat after Hoppe-Seiler's "Handbuch der physiologisch- und pathologisch-chemischen Analyse," but dealing only with plant products, there is a wealth of information in the 400 pages. Each group of plant constituents is discussed, first under the general group, then under the group subdivisions, and lastly each compound is given, its structural formula (when known), its properties, its chemical reactions, its micro-chemical reactions in many cases, the qualitative tests

for its presence, and the methods for its quantitative estimation. The quantitative estimation is illustrated in a majority of the cases by an example, so that the student can not go astray. Perhaps in some of these cases the calculations could have been omitted, for many are so simple that any one who could understand the directions should be able to calculate percentage, etc., but it is better to err in being too explicit rather than be too obtuse.

The literature has been well reviewed, but, unfortunately, the book contains no author index, so that the numerous author citations lose a very considerable part of their value. It is to be hoped that this feature will be remedied in a second edition.

The book is well printed on good paper, and is remarkably free from typographical errors. It should prove a useful volume to the average chemist, and invaluable to the plant physiologist or the teacher of plant chemistry, both as a reference book and as a text-book. Needless to add it should be in every chemical library.

Ross Aiken Gortner

SPECIAL ARTICLES

THE ORGANIZATION OF THE CELL WITH RESPECT TO PERMEABILITY

In studies on permeability it is assumed that we need consider but one surface, namely, the outer "plasma membrane." It seems desirable to emphasize that the problem really involves a variety of surfaces[1] the permeability of which may be decidedly different.

Good illustrations of this may be found in many kinds of plant cells. A very favorable object for investigation is afforded by the marine alga *Griffithsia*. Within the cell wall is a thin layer of protoplasm which surrounds a large central vacuole. The protoplasm therefore forms a sack which is filled with liquid. It is capable of expanding or contracting as water is taken up or withdrawn by osmotic exchange.

[1] The term surface is preferred, since a semi-permeable surface may exist where there is no definite membrane.

If these cells be placed in hypertonic sea water water is withdrawn from the cells and the protoplasmic sack contracts: on replacing the cells in sea water the sack expands to its original size. If in place of hypertonic sea water we use hypertonic NH_4Cl the sack likewise contracts, but the inner wall of the sack contracts a great deal more than its outer wall. The space between the two surfaces which is normally very small may increase until in places it equals one third of the length of the cell.

There are, therefore, two surfaces, the outer surface of the protoplasm ("plasma membrane") and the inner surface (vacuole wall) which do not act alike with respect to permeability. The interpretation of their behavior may be twofold. In the first place, the outer surface may be regarded as more permeable to NH_4Cl than the inner. The salt would therefore cause the outer surface to contract less than the inner since it is well known that the more freely a substance penetrates the less is its plasmolyzing power.

On the other hand, we may have to do with an alteration of permeability produced by the NH_4Cl. If the NH_4Cl produces an increase of permeability it may cause a contraction by what has been called false plasmolysis.[2] If the false plasmolysis of the inner surface is greater than that of the outer the effect which we have witnessed may result.

It is of course quite possible that both of these interpretations are correct and that we have both true and false plasmolysis contributing to the result. The writer is inclined to think that this is the case.

By lowering the concentration of the NH_4Cl we can produce a marked contraction of the inner surface while the outer still retains its full turgidity and shows no sign of contraction. This is most strikingly shown where a living cell adjoins a dead one. The turgidity of the living cell causes its end wall to bulge into the dead cell. As soon as the living cell loses its turgidity the end wall ceases to bulge and becomes nearly flat. It is therefore easy to determine whether the cell is turgid or not.

[2] Cf. *Bot. Gazette*, 46: 53, 1908; 55: 446, 1913.

Further experiments show clearly that false plasmolysis plays a part in this process, for hypotonic solutions or even tap water or distilled water may produce a contraction of the inner surface while the turgidity of the outer surface is maintained.

The chromatophores are numerous and lie embedded between the inner and outer surfaces of the protoplasmic sack. They contain chlorophyll and likewise a red pigment which is soluble in water. The red pigment is unable to escape from the chromatophore into the protoplasm under normal conditions because the surface of the chromatophore is impermeable to it. When the separation of the inner and outer surfaces of the protoplasm reaches a certain point the surface of the chromatophores usually becomes permeable to the red pigment so that it diffuses out. The cells then present a very striking appearance. The contracted vacuole remains colorless while all the space between the inner and outer surfaces of the protoplasm becomes deep red. The red pigment can not escape through the outer surface, nor can it pass through the inner surface into the vacuole. The cell may remain in this condition for an hour or two. Finally the red color begins quite suddenly to diffuse through both the protoplasmic surfaces.

The nuclei behave as though their surfaces were impermeable to the red pigment at the start, but they appear to become permeable to it soon after it begins to diffuse out from the chromatophores.

The cell wall which encloses the protoplasm is freely permeable to the red pigment and to salts at all times, but is quite impermeable to many other substances.

Similar effects have been observed in a variety of other cells.

Whether these effects are due to true or to false plasmolysis or to a combination of both, it is evident that the various kinds of surfaces (*i. e.*, the inner and outer protoplasmic surfaces, and those of the chromatophores, of the nuclei and the cell walls) can be proven to differ greatly in their behavior with respect to permeability.

The term differential permeability may be suggested as an appropriate designation of these phenomena.

The conception of differential permeability may perhaps be extended to surfaces other than those described here. Since the protoplasm is composed of a variety of structures (down to those which are ultramicroscopic) and each of these has a surface it is quite possible that many kinds of semi-permeable surfaces exist within the cell.

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THE SOCIETY OF AMERICAN BACTERIOLOGISTS. II

SANITARY BACTERIOLOGY

Observations upon the Bacteriology of the Baltimore City Water in Relation to the Typhoid Fever Present, and the Effect of the Hypochlorite Treatment: WILLIAM W. FORD and ERNEST M. WATSON.

Since October, 1910, up to the present time (December, 1912), a period of a little over two years, it has been possible for us to follow the bacteriological condition of the Baltimore city water by systematic examinations (weekly)—excepting for a brief period in the summer of 1911. These examinations have been of the nature of the bacterial count, the determination of the number of fermenting organisms present by means of the Smith tube, the isolation and determination of the various species present. The purpose of this work was (1) to determine the relation, if any, between the extent of the pollution and the amount of typhoid fever in the city, (2) to determine the seasonal variations in the bacterial content of the water and (3) to ascertain the effect of alum and hypochlorite of lime upon the city drinking water, as regards the bacterial content and later the effect of the purity or pollution of the water under these conditions upon the amount of typhoid fever in the city. It was found that in 1910 and 1911 there was a striking relation between the period of summer and fall pollution of the water and the summer rise in the amount of typhoid fever. The number of organisms in the water at this time ranged from 1,000 to 5,000 per cubic centimeter, and fermentation took place in 1/10 to 1/100 c.c.

and on one occasion in 1/1000 c.c. The entire significance of this relation will be fully determined only by further study. It further was found that the bacterial content of the city water during periods of pollution was different from that during periods of relative purity. *Bacillus coli* comprised 55 per cent. of all organisms isolated during the period of relative purity, while during the period of pollution it comprised only 25 per cent. of organisms present. At this latter time, however, several new forms made their appearance, such as *lactis aerogenes*, intermediate group, attenuated forms and "liquefying fermenters." During little more than a year now it has been possible to observe the effect of hypochlorite of lime and later of alum on the bacterial flora of the city water. In the main the bacterial count has been greatly reduced under the chemical treatment, the counts practically always being less than 500 organisms per cubic centimeter. Notwithstanding this, however, the degree of fermentation has remained practically the same, *i. e.*, 1/10 and 1/100 c.c. of the water giving positive tests in the Smith tube. The typhoid fever during this period of chemical treatment has been slightly reduced. However, the reduction was not at all striking, which makes us believe that perhaps the greater part of our typhoid fever may not be water-borne or, on the other hand, if water-borne, the specific organisms of pollution have not been removed from the water by the chemical treatment in the usual strengths of available chlorine.

Some Results of the Hypochlorite Disinfection of the Baltimore City Water Supply: J. Bosley Tomas and Edgar A. Sandman, Baltimore City Water Department.

Stokes and Hachtel[1] have reported the result obtained by the hypochlorite disinfection of the Baltimore city water supply during a period extending from the institution of the treatment on June 15, 1911, to October 30, 1911. They examined samples taken from the untreated water in the impounding reservoir and from the treated water after it had passed through each of two storage reservoirs. The result of their examination showed bacterial reduction varying between 94.5 and 99 per cent. They also showed average reduction in the colon bacillus from 57.5 per cent. positive tests with 0.1 c.c. of untreated water to 12 per cent. positive tests with 0.1 c.c. of treated water, and from 89 to 40 per cent. with 1 c.c. The greatest reductions were obtained with one part per million of available chlorin, when there were shown reductions from 86 per cent. positive tests with 0.1 c.c. of treated water, and from 100 per cent. to 37 per cent. with 1 c.c. The period covered by the following report extends from January to December, 1912. In addition to the places sampled by Stokes and Hachtel we obtained samples at the influent of the first storage reservoir, after the water had passed through seven miles of tunnel subsequent to treatment. The time required for the water to pass through this tunnel varies between 4.9 and 12.2 hours. While allowing sufficient time for effective disinfection, the taking of samples just before the water enters the first storage reservoir permits of counts being obtained before any after-growths are likely to have occurred. The amount of available chlorin applied during the period covered by the report of Stokes and Hachtel was raised from 0.4 parts per million applied at the start on June 15, to 0.6 on June 23 and to 1 on October 15. On July 15, 1912, the amount was again raised, by order of the Commissioner of Health, to 1.5 parts per million, and this amount has been maintained until the present time. From January 11 to November 12 aluminum sulfate, in amounts varying between 0.610 and 1.05 grains per gallon, was applied to the water as it entered the first storage reservoir. Shortly after the period covered by the report of Stokes and Hachtel after-growths in the storage reservoir caused excessive bacterial counts. These conditions maintained during the first five months of the year, but about the middle of May the counts showed a marked diminution, and no further after-growths were observed, excepting during the few days in September. The monthly averages of the results in bacterial counts and *B. coli* tests, shown in the accompanying table, are taken from daily analyses. The counts during the first six months were obtained on standard agar at 20°, and during the remainder of the year at 37°. The *B. coli* averages were obtained from tests made on portions of water varying by a multiple of ten from 0.001 c.c. to 100 c.c., sufficient number of tubes being used in each case to secure at least one negative and one positive test, excepting when no fermentation was obtained with 100 c.c. The average number of *B. coli* per cubic centimeter for each month was estimated by considering the number of positive and negative tests in each dilution and following the method described by Phelps before the American Public Health Association in 1907. Lactose bile was used as an

[1] *Am. Jour. Pub. Health*, April, 1912.

initial medium, and Endo's agar was used for isolating the members of the *B. coli* group in pure culture, nearly 100 per cent. successful isolations having been obtained by the use of this medium, whereas the frequent encountering of spreaders on litmus agar and the fact that many of the acid-forming colonies proved not to be members of the colon group seriously impaired the efficiency of this latter medium. No attempt was made until in the last two or three months to differentiate the four members of the colon group; but this is now being done with the use of dulcit, in addition to the usual sugars, and morphological examinations, and the results seem to show a greater vulnerability of the two *B. coli* organisms than of *B. aerogenes* and *B. acidi lactici*. The results obtained by the use of the 20° temperature show much greater reduction in the bacterial count than those obtained with the 37° temperature, and we believe that counts should be made at the higher temperature in addition to those made at 20°. The effects of the treatment of this water supply have been a very good reduction in the bacterial count of the water as it enters the first storage reservoir, and almost entire elimination of the members of the *B. coli* group, the treated water during three months showing none of these organisms at any time in 100 c.c. The reduction in the number of cases of typhoid fever occurring in Baltimore during 1912 is 31 per cent., compared with an average of the number of cases occurring during the years from 1906 to 1910, and 24 per cent., compared with the number of cases occurring during 1911, in the last six months of which the water supply was treated. We wish to acknowledge indebtedness to Mr. Ezra B. Whitman, water engineer, and to Mr. Emory Sudler, engineer in charge of the improvement of the water supply, for an interest unusual with the engineers not directly acquainted with the details of the laboratory work.

Experimental Disinfection of Water with Calcium Hypochlorite (preliminary note): F. W. HACHTEL, M.D., and RAYMOND FREAS, A.B., Bacteriological Laboratory of the State and City Boards of Health, Baltimore, Md.

The following brief report is made upon certain experiments that were begun in the midsummer of 1911 and which have for their object the determination of the amount of available chlorine necessary to eliminate the *B. coli* from 10 c.c. of water under varying conditions of turbidity. They were instituted because, although the quantity of available chlorine added to the Baltimore drinking water had gradually been increased from 0.4 to 0.75 parts to the million gallons, the colon bacillus still persisted with too great frequency in 1 and 10 c.c. of water collected at the storage reservoirs and as drawn from the taps. The first series of experiments had to be done in a hurry, as heavy rains on the watershed were markedly increasing the turbidity. At this time, therefore, only presumptive tests in lactose bile were done. This, however, is not a guide to the sanitary condition of water treated with hypochlorite of calcium as shown by some work carried on in the laboratory on samples collected from the taps and storage reservoirs after treatment. As a result of these we found that although they would not infrequently produce gas in lactose bile, we were unable to obtain the colon bacillus in pure culture even after repeated platings in lactose-litmus-agar. A number of these were then plated out anaerobically and in a considerable percentage of cases we obtained *B. welchii* or the *B. sporogenes* or both. This, therefore, led us to repeat our work, and the results may be summarized as follows: With a turbidity of 32 we have found that 0.75 and 1 part of available chlorine to the million gallons caused a bacterial reduction of about 80 and 90 per cent., respectively, in six hours. During the same period the bacterial content of the untreated water was doubled. At the end of twenty-four hours, although the untreated water showed a count of 300 times as great as when the experiment was started, the two treated waters gave counts of only 0.3 per cent., as great as that of the raw water at the beginning. Again, the water before treatment contained the colon bacillus in 1 c.c. but not in 0.1 c.c.; at the end of one hour after the addition of calcium hypochlorite in the aforementioned quantities 1 c.c. of the treated water failed to ferment. In addition, although there was gas formation in all the lactose-bile tubes inoculated with 10 c.c. for the first six hours after treatment, nevertheless the colon bacillus was not isolated from any of these, though they were plated out on three consecutive days. On the other hand, we were able to obtain *B. welchii* or *B. sporogenes* from almost all of them. Four, five, six and twenty-four hours after the addition of hypochlorite 50 c.c. of each of the treated waters were inoculated into large tubes of lactose-bile and although fermentation occurred in all save one, *B. coli* was not obtained from any of these in spite of repeated attempts. In every

instance but one *B. welchii* was present. It should be stated that the water was kept in the dark and at the out-of-door temperature. The calcium hypochlorite used had 34.1 per cent. of available chlorine. It is worth noting that in one experiment with water of a turbidity of 12 in which we used 0.75 part of chlorine per million the colon bacillus was present in 1 c.c. at the end of two hours and in 10 c.c. at the end of three hours. In addition to this, 10 c.c. of the treated water still caused fermentation at the end of six hours, but not after twenty-four hours. The colon bacillus, however, was not isolated from any of these. In this instance the bacterial content of the water was about one seventh as great as in the previously described case. This result is to be ascribed to the very low available chlorine content of the hypochlorite used —this being only 1.546 per cent. We have been unable to repeat the experiments with water of very high turbidity, owing to the lack of heavy rains on either of the two watersheds, but we purpose to do so at the first opportunity. Besides this we propose to determine if there is any relation between the temperature of the water treated and the amount of chlorine necessary to destroy *B. coli*. So far we can but state that, with water of a turbidity of about 30, a bacterial content of 15,000 and the colon bacillus present in 1 c.c. and not present in 0.1 c.c., 0.75 part of chlorine to the million gallons eliminates *B. coli* from 10 c.c. in one hour and from 50 c.c. certainly in four hours and possibly in less time; of course this presupposes the use of hypochlorite of high available chlorine content.

The Distribution of B. coli in Polluted Oysters: John W. M. Bunker, Ph.D., instructor in sanitary analysis, Harvard University.

To establish whether the distribution of *B. coli* in polluted oysters is or is not uniform throughout the regions of the oyster body, examination was made of the following regions of 145 oysters taken from regions subject to varying conditions of pollution in Narragansett Bay: shell liquor from the branchial chamber, material from the mouth, material from the stomach, material from the intestine at the point where it bends sharply upon itself, material from the extremity of the intestine, shell liquor from the cloacal chamber, decanted mixed shell liquor. As a result of these examinations it is evident that (1) the distribution of the colon bacillus is not uniform throughout the various regions of a polluted oyster; (2) of the body regions, the stomach, in general, contains the colon bacillus most frequently; (3) at all seasons of the year the colon bacillus is found more frequently in the shell liquor than in any portion of the body; (4) when the temperature of the water on the oyster beds is below from 6°to 8° C., the best index of pollution as afforded by the *B. coli* test can be obtained from the liquor in the cloacal chamber; (5) at temperatures of above 8° C. the liquor in the branchial chamber is the most reliable source of information regarding pollution; (6) at no season of the year does the practise of decanting the shell liquor afford the most reliable index of pollution that could be obtained.

The Bacteriology of the Hen's Egg: Leo F. Rettger, Sheffield Scientific School, Yale University.

In our investigations of bacillary white diarrhea in chicks we have made bacteriological examinations of at least ten thousand eggs. While our chief object was the detection of *B. pullorum*, the specific cause of the disease, a general bacteriological study was made of the eggs, and particularly those which were fresh and apparently normal. Until the spring of 1912 the yolks only were examined, as a rule. During the past year special tests were made with the whites. In the examination of the yolks of fresh and unincubated eggs the entire yolks were employed. They were removed aseptically and mixed in special test tubes of large diameter with 25 cubic centimeters of plain bouillon. The tubes were kept three to four days at 37° C., and for an additional period of twenty-four hours at 20°. Streaks were made with platinum loops on slant agar. Incubated eggs were tested directly, that is a small amount of the yolk was streaked over the surface of slant agar. In the testing of whites 5 cubic centimeters of the egg-white were mixed with 100 c.c. of sterile tap water. These tests were made in duplicate. One flask of the diluted white was kept for five to six days at 20° and the other at 37°. Slant agar streaks were then made. From the results of the numerous tests we were led to conclude that the yolks and whites of fresh eggs were, as a rule, sterile. Among the organisms found (aside from *B. pullorum*) the most conspicuous was a large spore-bearing bacillus, resembling in many ways *B. mesentericus*. In addition to this the following were observed: *Proteus vulgaris, B. pyocyaneus, B. fluorescens, B. coli,* cocci and moulds. It is quite probable that many of the organisms obtained in the tests were contamination forms. Eggs which were incubated artificially for from

one to three weeks seldom gave us any indications of containing bacteria. The only organism which could be regarded without doubt as coming from the interior of the egg was *B. pullorum*, and this was always found, when present, in the yolks of both fresh and incubated eggs. The results have been quite different, however, with eggs that were kept in warm, damp places for any length of time, and those which were left for several days under sitting hens. Such eggs, especially the infertile, frequently contained bacteria.

On Antiseptic and Bactericidal Properties of Egg White: JOEL A. SPERRY, 2d, M.S.

The white of the eggs was aseptically transferred to sterile test tubes in 5 cubic centimeter quantities and then inoculated with various organisms. Small amounts of the egg white were introduced into dilution flasks and agar plates were made with 0.5 c.c. of the dilution. The egg white showed strong bactericidal properties toward *Subtilis cereus* and *megatherium* while towards *coli, typhi, anthrax, Proteus vulgarus, Staphylococcus pyogenes aureus* and other organisms the antiseptic action only was noticeable. This was true for the white of fresh eggs and cold-storage eggs not more than nine months old. The action of egg white on *putrificus*, malignant edema and symptomatic anthrax seemed to be purely antiseptic. The white of eggs which are eleven months old or more showed a tendency to lose these properties.

SOIL BACTERIOLOGY

A New Method for the Bacteriological Examination of Soils: P. E. BROWN, Iowa State College, Ames, Iowa.

A brief statement of the situation regarding the bacteriological examination of the soils brings out as salient points that the mere quantitative examination of soils is of little value from the fertility standpoint; that the logical means by which conclusions can be reached concerning the influence of varying bacterial content on crop production consists of certain groups of organisms as measured by the chemical products of their growth and actual crop production; and that a necessity therefore for progress in the work is the formulation of satisfactory methods for measuring the activities of certain important groups of soil organisms. A discussion of the methods previously employed while recognizing certain value attached to the results obtained thereby, points out the objections to the solution method and to the use of sterilized or air-dry soil as media and the conclusion is reached that fresh soil is the logical medium to be employed. Plots differentiated through special treatment were employed in experiments and satisfactory results were secured using fresh soil with ammonium sulfate for nitrification and fresh soil with mannite for azofication. For ammonification more difficulty was experienced in selecting a suitable nitrogenous material to permit of an accumulation of ammonia in sufficient amounts to be measured. Comparisons of the results obtained using air-dry soil and infusions of fresh soil and dried blood, albumen and casein with those secured using fresh soil and the same nitrogenous materials showed that casein added in solution to fresh soil brought out the greatest differences in the ammonifying power of the soils and possessed also certain other advantages incident to manipulation. The method recommended consists then in testing of fresh soil obtained as described in previous work by the writer, adding a solution of casein for ammonification, ammonium sulfate for nitrification and mannite for nitrogen fixation.

A Cultural and Morphological Study of some Azotobacter: DAN H. JONES, Ontario Agricultural College, Canada.

From various samples of soil taken from the garden of the Ontario Agricultural College, sixteen colonies of *Azotobacter* were isolated. A study of these cultures extending over two years shows them to comprise four distinct varieties or species. These have been tentatively named *A*1, *A*2, *A*3 and *A*4. *A*1 and *A*2 bear a resemblance to *Azotobacter chroococcum* and *A*3 and *A*4 bear a resemblance to *Azotobacter agilis*, as described by Beyerinck. All cultures in Ashby's solution fix atmospheric nitrogen in the form of nitrates. In young cultures (one to two days old) of each variety, the organism is a short, thick rod with rounded ends, frequently occurring in diplo form and motile by means of peritrichic flagella. At this stage, the internal protoplasm is homogeneous, though occasionally what may be a nucleus in the form of a spherical granule is present, this undergoing a fission when the cell divides. When cultures are four to five days old, the cells become irregularly spherical, coarsely granular and nonmotile. The granules enclosed are spherical, vary in size and number and are often of two kinds. The one kind of granule gives the glycogen reaction when treated with iodine-potassium-iodide solution, but is negative to certain anilin dyes, whereas the other kind is negative to the glycogen

stain but positive to the anilin dyes. This second kind of granules appear to arise from the aforementioned nuclear body, and the first mentioned kind appear to be a product of the cell activities, possibly a reserve food supply. In active cultures from five to ten days old, many of the cells disintegrate, their enclosed granules being scattered. The granules of the second type appear to give rise to new organisms, acting in this particular as gonidia, while those of the first type slowly disappear as though they were dissolved. At this stage, *A*1 and *A*2 produce large capsules, but *A*3 and *A*4 do not. Fission frequently takes place within these capsules, thus producing an irregular group of from two to six or more organisms within a capsule. When cultures are about three weeks old, the majority of the organisms appear as spheres, *A*1 and *A*2 in irregular clusters, *A*3 and *A*4 in fairly regular packet and sarcinæ forms. This condition occurs only when the cultures are near their full development and appears to be a resting stage. Chains of from four to thirty cells are common in old liquid cultures as Ashby's solution. Involution forms of a great variety in size and shape appear in old cultures, but the most striking changes in morphology occur in cultures incubated at 37° C., especially in case of *A*1, in which many of the cells elongate into tubes 40 or 50 μ long. Colonies and streak cultures on Ashby's agar are first hyaline, then white and when they are fully developed a brown pigment is produced, which in case of *A*2, *A*3 and *A*4 in time frequently becomes black. Mass cultures of *A*1 are very moist and have a tendency to flow; those of *A*2, while being moist, do not flow, but become contoured in topography; those of *A*3 are pasty; those of *A*4 somewhat coriaceous and verrucose. Ashby's media give the best growth, beef extract media allowing but a restricted development. Good growth on Loeffler's blood serum.

The Origin of Certain Organic Soil Constituents: M. X. Sullivan.

Examination was made of the dried mold, *Penicillium glaucum*, grown on Raulin's solution and of the filtered solution after mold growth for organic constituents. In the alcoholic soda extract of the mold were found oleic and palmitic acids and a fatty acid melting at 54° C., hypoxanthine, guanine, and adenine, histidine, thymine, choline, probably lysine and a small amount of hydroxy-fatty acids. In the direct alcohol extract was found mannite, cholesterol bodies, hypoxanthine and cerebroside. In the culture solution were found fatty acids, guanine, adenine and hypoxanthine, a small quantity of histidine, pentose sugar, unidentified aldehydes and a small amount of hydroxy-fatty acids. Most of these compounds have been found in soil and the conclusion is made that in the formation of the various organic soil constitutents, microorganisms, such as yeasts, bacteria and molds, play an important part.

Soil Inoculation under Soil Conditions of Lime Deficiency: T. D. Beckwith.

The Cascades divide the state of Oregon roughly into two sections differing greatly as to rainfall and consequent seepage of soluble soil constituents. Much of the land in the Willamette Valley and western section of the state has a lime deficiency of from one to five tons per acre-foot. With the idea of learning whether or not artificial inoculation of legume seed with pure cultures of *B. radicicola* might be expected to yield results, reports of success or failure of soil inoculation cultures furnished by the department of bacteriology have been sent to Oregon Agricultural College, accompanied by root specimens. During the past summer 110 tests have been carried out, at least 60 of which have been with alfalfa. A compilation of the results obtained shows that the method was beneficial in 69 per cent. of the experiments. On the contrary, of 50 tests carried out in the eastern part of the state in soils well furnished with lime, success was obtained in 45 instances, or 90 per cent. It is thus evident that *B. radicicola* may retain virulence to the roots of legume plants, under conditions of a small amount of soil acidity. Results were unfavorable when lime deficiency was over five tons per acre-foot.

Bacterial Activity in Soil as a Function on the Various Physical Soil Properties: Otto Rahn, University of Illinois.

To study the influences of physical soil properties upon bacterial activity in soil, pure cultures of *B. mycoides* in quartz-sand peptone water mixtures were studied. In one series, cellulose was added to the sand. The amount of ammonia formed under these conditions was taken as the indicator of bacterial activity. Further, *Bact. lactis acidi* was grown in milk sand mixtures, acidity and number of cells serving as measure of development. The conclusions are greatly influenced by the basis of comparison. If the data are computed per 100 g. of dry soil, as is customary among soil bacteriologists, it would seem that the bacteria thrive best in a fairly moist sand (20–25 per cent.). If, however, the actual culture

medium, *i. e.*, the peptone solution, is used as basis, ammonification is most rapid in the driest soil (10 per cent. water). If the data are computed per 100 c.c. of soil solution, the concentration of the solution is again of greatest importance. The results vary greatly if one time the peptone is given in proportion to the amount of soil and another time in proportion to the amount of soil moisture. The farmer is primarily interested in the amount of plant food per weight of soil; the efficiency of bacteria can be determined only by comparing equal amounts of culture medium and of food. In test tube or flask cultures of *B. mycoides*, oxygen is always in the minimum. In sand cultures, the oxygen exchange is greatly increased and the rate of development is correspondingly higher. The oxygen exchange between gas and liquid depends upon the oxygen content of the soil air and upon the surface exposed to this air. The surface per unit of liquid is inversely proportional to the diameter of the soil particles and to the moisture content of the soil. The oxygen content of the soil air depends upon the ventilation which is nearly proportional to the square of the grain diameter. A thinner film of moisture gives therefore a faster decomposition, but there is a limit to the thinness of this film, extremely thin films causing a retarded decomposition. The optimum thickness of moisture film in the case of *B. mycoides* was between 20 and 40 microns. This film was obtained in sand of 1 mm. diameter at a moisture content of about 10 per cent. In arable soils, with a grain size not more than 0.1 mm., it would require more than 50 per cent. of moisture to produce the optimum film thickness. In other words, strictly aerobic bacteria will never find optimum conditions of existence in soils. The ultimate endpoint of decomposition, if the food concentration was constant, was the same in the case of *B. mycoides*, since only the rate of decomposition was influenced by the efficiency of the oxygen supply. With some other bacteria, the endpoint varied greatly. The behavior of anaerobic bacteria, represented by *Bact. lactis acidi*, was in accordance with the above-mentioned principles, the main factor for their development being a very thick moisture film. The physical effects of undecomposed organic matter were imitated by the addition of finely ground filter paper to sand. In fairly dry soils, cellulose caused a decrease of ammonia formation by making some of the soil moisture unavailable for bacteria. In the moisture sands, cellulose increased the ammonification probably by holding the sand particles farther apart and thus increasing aeration.

Characteristics of Cellulose-destroying Bacteria: I. G. McBETH, F. M. SCALES and N. R. SMITH.

Seventeen species of cellulose-destroying bacteria have been isolated and studied; 7 of these belong to the genus *Bacillus*, 4 to the genus *Bacterium* and 6 to the genus *Pseudomonas*. All are morphologically and physiologically different from Omelianski's hydrogen and methane ferments. None of the species studied have shown any tendency to form gaseous products, and in relation to oxygen all are facultative aerobes. By means of cellulose agar colonies the species may be separated into two distinct groups: those forming opaque colonies which clear a well-defined zone beyond the colony and those which form transparent colonies with little or no indication of an enzymic zone. All of the organisms grow more or less rapidly on beef gelatin, but only 10 of the 17 species studied have shown any power to liquefy gelatin. On beef agar 11 species grow rapidly and luxuriantly, 4 species grow poorly and 2 have failed to give any growth at all. When introduced into Dunham's solution 9 species have shown the power to form ammonia. The action on litmus milk is also quite variable; 10 species give an acid reaction, 5 an alkaline reaction and 2 make no growth. The digestion of the milk occurred with only 4 species. Eleven species have shown a growth on potato cylinders while 6 have shown no growth or only a slight bleaching action along the track of the inoculum. The action of the cellulose-destroying bacteria studied shows marked differences in their activity toward the other carbohydrates such as dextrose, lactose, maltose, saccharose, glycerine, mannite and starch in peptone solutions. In their relation to these solutions the cellulose-destroying organisms may be divided into the following groups: (1) those which give an acid reaction from all of seven peptone carbohydrate solutions used; (2) those which give an alkaline reaction from all of the peptone carbohydrate solutions; (3) those which give an acid reaction from only a part of the peptone carbohydrate solutions; (4) those which produce no change in the reaction of any of the peptone carbohydrate solutions.

A Plan for Revivifying Bacteria by Groups: H. J. CONN.

Our present standard method of revivification, in non-saccharine broth at 37°, is not applicable

to most soil organisms nor to many other bacteria. A possible standard method is here suggested for revivifying the bacteria that do not grow under such conditions. The bacteria are to be divided into five groups:

1. Growing well in plain broth at 37° C.
2. Excluded from group 1, but growing well in plain broth at 20° C.
3. Excluded from groups 1 and 2, but growing well in dextrose broth at 37° C.
4. Excluded from groups 1, 2 and 3, but growing well in dextrose broth at 20° C.
5. Excluded from all four groups, but growing well on surface of agar.

Each of these groups is to have its own method of revivification, as follows: 1, in plain broth at 37° (as at present); 2, in plain broth at 20°; 3, in dextrose broth at 37°; 4, in dextrose broth at 20°; 5, on agar slants. This classification includes most soil bacteria and many others; but further groups may be added as they prove necessary. These groups are somewhat similar to the groups of the bases recognized by chemists in qualitative analysis. Like the chemical groups, they are to be disregarded after the unknown has been determined.

The Ammonifying Efficiency and Algal Content of Certain Colorado Soils: Walter G. Sackett.

The power to transform organic nitrogen into ammonia is a property common to many cultivated Colorado soils. Soils in the incipient stage of the niter trouble appear to surpass our normal soils in ammonifying efficiency. Compared with soils from other localities, our niter soils excel in ammonifying efficiency to a very marked degree. Nineteen of the thirty-one soils examined have ammonified cottonseed meal more readily than the other nitrogenous materials employed; the remaining twelve have broken down in the dried blood most easily; twenty-six have formed ammonia from alfalfa meal more readily than from flaxseed meal, and with five the reverse has been true. The maximum per cent. of ammonia produced in seven days by any soil from 100 mg. of nitrogen as cottonseed meal was 51.98 per cent.; as dried blood 52.64 per cent.; as alfalfa meal 34.85 per cent.; as flaxseed meal 12.15 per cent. Algæ occur abundantly in many cultivated soils of Colorado. Twenty-one different species of algæ were found in the soils examined. With but two exceptions, all the species found belong to the blue-green algæ (Cyanophyceæ.) The family Nostocaceæ is best represented. There is a predominance of forms possessing thick, gelatinous sheaths. This paper is published in full as Bulletin 184 of the Colorado Experiment Station, Fort Collins, Colorado.

Nitrogen Fixation by Organisms from Utah Soils: E. G. Peterson and E. Mohr.

This paper is a preliminary note in a proposed extensive investigation regarding the fixation of nitrogen in Utah soils and the rôle played by microorganisms in this action, together with the various agencies influencing bacterial action. Samples of soil from which the organisms described were isolated were taken weekly from January 9 to November 4, 1912, from Greenville Experiment Farm, Utah Experiment Station. 100 c.c. portions of mannite solution were inoculated with 10 grams of soil and incubated at 20° C. After ten days' incubation subcultures were made in mannite solution and incubated for ten days at 20° C. Isolations were made from plates which were made from these subcultures. Several types of colonies were formed, but only three appeared that grew readily and for a long period on mannite agar. The paper describes these three forms. One of the three forms was undoubtedly *Azotobacter chroococcum*, the other two heretofore undescribed in western soils. Type No. 1 fixed 5.335 mg. of nitrogen in twenty days in mannite solution, average of 15 tests; type 2 (*Azotobacter chroococcum*) fixed 5.616 mg. of nitrogen in twenty days, average of 10 tests; type No. 3 fixed 5.588 mg. of nitrogen in twenty days, average of 12 tests. Analyses were made from January 9 to October 28 to determine if possible any marked seasonal variations in nitrogen fixation. The technique involved the addition of definite quantities of soil, taken under standard conditions, to mannite solution, the amount of nitrogen in the soil being subtracted from the amount of nitrogen present at the end of twenty days in order to determine the amount fixed. The variation was found to be very marked from week to week without apparent regularity, a marked increase in fixation power being noted from the middle of May to the end of June. Isolations were made from these impure cultures to determine the presence of the three colony types described in the paper. Types No. 1 and 3 were present in the majority of samples, type No. 1 predominating in all cases. Type No. 2 was present once in April, twice in June and once in September. Further work is being done on the three forms isolated.

A. Parker Hitchens,
Secretary

(To be concluded)